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PERCEPTION AND PERSONALITY

A Symposium

PERCEPTION AND PERSONALITY

A Symposium

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PREFACE

It is perhaps a mark of the maturity of a science that discontinuity between its various bodies of data ceases to exist. There is not one set of principles to describe, say, the phenomena of light; another to describe the phenomena of electromagnetism. With maturity, common principles encompass what were before discontinuities in knowledge.

We believe that this process of maturation is moving at a rapid rate in the field of psychology. The subfields within psychology are beginning, at long last, to lose their identity. Theory and research in social psychology have a determinate reference to work on personality, on psychophysics, on the development of the child. Common principles gradually emerge. Their emergence is not adventitious, no happy accident. Nor are the principles arrived at by fiat or by vote, the product of composing divergent views around a committee table. Common principles emerge because different approaches and different perspectives are brought to bear on a common segment of behavior.

The manifold attack on the processes of perception represented by the articles gathered in this book is, we think, an example of how psychology may be unified. Here we see experimental psychologists—traditional "perceptionists"—social psychologists, students of personality, clinicians, all concerned with the determinants of perceptual operation. It is not that the authors, like Molière's hero awakening suddenly to the ubiquity of prose, have precipitately realized that everybody perceives, even people with personalities. Rather what has dictated the study of perception as a common focus is its fruitfulness. The prediction of behavior, particularly complex behavior, is cripplingly incomplete without an account of the perceptual field of the predictee. And so many of the controlling principles of behavior manifest themselves in changes in the perceptual field.

There is something else about the study of perception. There is an appeal in perceptual theory which is as strong a compelling point

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as the logical importance of the perceptual process itself. It has attracted the social psychologists, investigators of personality, and clinicians to the study of perception and to the belief that therein will be found a possible unifying set of concepts for all psychologists. Perceptual theory, especially in the hands of the *Gestaltists,* has already been applied to cover a multitude of phenotypically different processes—perception, learning, and forgetting, to mention a few—and the generality of these organizational principles has an attracting power for the theorist of almost every field of psychology.

While perceptual research leads to the unification of psychology, the papers in this book amply demonstrate that unification is only in its initial stages. How is this unification to take place? There is no agreement. Some authors call out for a "co-operative" approach between perceptual theory and motivational theory. Others engage in a jurisdictional dispute, insisting that either perceptual or motivational theory take the right of way. Still others would revise *both* sets of theories, striving for an entirely "new" and general set of concepts. In our opinion this lack of unanimity is in the best traditions of unification. The very fact that here, for almost the first time, the data *and* theories of social psychologists, clinical psychologists, and perception psychologists are actually joined in issue, actually cross each other, is a highly encouraging event. The era of splendid isolation seems to have gone.

But if one of the theses of these papers is that perceptual principles have much to contribute toward the unification of psychology, then the second thesis seems to be that this promise will be fulfilled only if perceptual theory itself is revised. And here even the most "imperialistic" perceptionists seem to agree. For this is not to be a venture in premature generalization. Psychology gradually learns that "overunification" can be more a hindrance than an aid. Overunification under the banners of CR or, before that, instinct, did us little service. There is no proposal in these pages that all other forms of behavior be reduced to a few principles of perception. There is no threat to make the principle of closure as ubiquitous as the conditioned response.

It is heartening to note, among many of the papers included in this volume, a critical and frequently iconoclastic approach to the very principles of perception which are seen, at the same time, as the great hope of psychology. The personologists and perceptionists represented here are not merely taking over the perceptual laws and principles which have come from the perceptionist's laboratory. One gets the impression, from reading the experimental reports here, that instead of "receiving" these principles graciously from the perceptionist, the social and clinical psychologists are invading the perceptionist's laboratories and are re-examining the principles from their own vantage point, subjecting them to further experimental investigation, and are ending by "motivating" and "socializing" the original principles. No law or generalization seems to be left in peace in this search for more adequate perceptual principles to apply to social and clinical data.

The editors feel that the papers embodied in this volume represent an important step forward in psychology. The approach represented serves to make the clinician aware of the potentialities of the laboratory—not only its rigor but its flexibility. The experimentalist, for his part, is made to realize anew that his subjects have personalities and aspirations and anxieties, that they live in society and that all these facts influence in critical ways their behavior. Calling a person an S or an O or a Vp makes him no less a person, his behavior not a whit more desiccated. It is self-defeating to "randomize" or "hold constant" these large and important determinants of perception or behavior generally. And the experimentalist is learning to study these personality determinants.

In the hope that these papers may stimulate more work along these lines, this volume is being published in as convenient a form as possible. The papers appeared originally in the September and December, 1949, issues of the *Journal of Personality* (Vol. XVIII, Nos. 1 and 2). We are grateful to the editor of the *Journal,* Karl Zener, and to its publishers, the Duke University Press, for their aid in bringing out these papers in this more accessible form.

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Cambridge, Massachusetts

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Berkeley, California

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PERCEPTION AND PERSONALITY

A Symposium

PART I

EDITORIAL NOTE

The present and the succeeding issue of the *Journal of Personality* constitute two parts of a symposium devoted to various phases of the general problem of the functional interrelationships between the process of perception and its determinants in personality. Increased recognition of the intimacy and significance of these relationships has led, among clinicians, to an enhanced stress upon technical diagnostic and therapeutic methods based upon them, and, among experimentalists, to a renewed outburst of research directed towards an analysis of the basic processes involved.

Theoretical aspects of the problem area were explored in the recent Symposium on Personal and Social Factors in Perception held at the Denver meetings of the American Psychological Association. It was felt that the intrinsic significance of the problem area and its current research exploitation made desirable immediate availability of the papers there presented. The present September issue of the *Journal* contains, accordingly, the three papers presented at the symposium, with the introductory remarks and subsequent discussions and also three related theoretical papers. The following December issue will contain a series of research reports on special problems within the general area.

In an attempt to encourage such a co-ordinated constructive approach to other problems the *Journal of Personality* will from time to time contain other such symposia. Preferably they would be concerned with problem areas in which significant advances at both the conceptual and research levels are in progress.

KARL ZENER

INTRODUCTORY REMARKS

HEINZ WERNER
Clark University

The last two decades have witnessed a shift in the status of perception within psychology as a whole. This shift is primarily due to the growing recognition of what one might call the projective nature of perception or, to use Gardner Murphy's formulation, to the recognition that "the perceived world pattern mirrors the organized need pattern within" (7, p. 351). The expansion of the problem of perception from the splendidly isolated area of classical psychophysics to the areas of social and personality dynamics necessitates a reformulation of the theoretical framework within which perception is to be conceived.

Paradoxically enough, the most abstract theory of perception, that of the nineteenth-century psychophysicists, and the naïve view of the man-in-the-street are in some ways quite similar; that is, both are sure that we see with our eyes and hear with our ears. It is interesting to note that people of "psychological intuition," such as literary writers and artists, hardly ever subscribed to such a view, since it always has been their business to perceive with what they would only metaphorically refer to as their eyes and ears. After Goethe visited the Dresden Gallery with its marvelous collection of Dutch landscape paintings, he remarked how for days the visible world had transformed itself for him, how he saw things and sceneries in the mood and—metaphorically speaking—with the eyes of the Dutch artists. Perception psychologists of today might read with considerable profit Stendhal's book *De l'amour* (9), in which that great French literary psychologist of the early nineteenth century analyzes the changes that occur in the lover's perception of his object of love. Stendhal employs an interesting concept for his analysis, that of "crystallization." Leaving a small branch in a salt mine, one finds that after a few days the branch has been transformed into a structure of innumerable little brilliant crystals that more or less obscure the original, rather commonplace, object. The theme of Stendhal's book concerns the stages and intricacies of the perceptual and imaginative processes that occur during that transfiguration of the love object by the lover. Charlie Chaplin has impressively demonstrated to millions of moviegoers that needs are organizing factors in perception by that unforgettable scene in *The Gold Rush* in which the starving companion perceives Charlie Chaplin hopping around as a fat and juicy oversized chicken. That such demonstration of perceptual behavior in spite of its abnormal exaggeration and bizarreness has its realistic angles has been quite recently brought home to me by observations made in a summer camp of adolescent diabetics.[1] The pictures of pin-up girls that, with youngsters of this age, usually cover the walls, were here replaced by colorful pictures of food: meat, sausages, and loaves of bread. This observation is, of course, well in line with the experimental studies on the effect of food deprivation on perceptual and imaginative processes, instigated by Sanford (8), Levine, Chein, and Murphy (4), by McClelland and Atkinson (6), and, in its wider aspects, with all the work on need factors in perception.[2]

The reasons why such studies are quite compatible with the artist's view and, at the same time, acceptable to the fellow-scientists, lie in the very nature of perception itself. Perception occupies an intermediate position between simple and complex behavior, or, as sometimes called, lower and higher functions. Perception has a methodological virtue in common with the "sensations" of classical psychophysics, namely, precise manageability in experimental design. But it also has the virtue of greater complexity and nearness to reality of life. It is this twofold characteristic, not attained by any other psychological function, that is rapidly making the area of perception a meeting ground of general experimental and clinical-social psychology.

The moving of perception into a central place carries considerable responsibility for the workers in this field toward psychology as a whole. If properly guided, perception psychology might help to stem off that "nasty fission" (10) in our science that many of us dread. It is, I am confident, this responsibility toward the whole of psychology which has brought together the men who, as outstanding contributors in the field, are going to discuss in this symposium some of the crucial problems.

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[1] This observation was made by L. McKeeman.

[2] See the literature cited in the symposium paper by Bruner and Postman in this issue.

This sense of responsibility is evidenced by a wholesome dissatisfaction with the status of theory in a field so greatly changed by new facts. There have passed now many years of intensive studies on projective perception. There have been unearthed many facts concerning need, motivational, and personality trait characteristics as linked to attributes and content of perception. But little has been done in the past to dignify that work by the formulation of testable hypotheses about basic mechanisms underlying these relationships, hypotheses that would tie together the general psychology of perception and these newfangled, personological findings.

The attempts to integrate facts and principles of general and motivational psychology will, of course, vary considerably in view of the wide differences in background and temperament of the individual psychologists. These attempts will vary in scope; but psychologists will also differ as to the functional level of the organism at which a unifying interpretation is to be undertaken.

Some of us are convinced that unifying principles or mechanisms should be sought now at a very basic neuropsychological level. Here the attempt would be to avoid the troublesome dichotomy between sensory factors, with which classical psychophysics deals, and need factors, by postulating that even the primary psychophysical responses to stimuli are not isolated sensory processes but more inclusive organismic activities. Thus, Wapner and I have presented a testable theory that postulates primordial interaction of sensory and tonic factors (11).

Others have attempted unifying interpretation at a somewhat higher level of autochthonous perceptual organization. One may claim, as Kurt Goldstein (2) has done, the figure-ground principle as a general law of organismic behavior. Or one may expand other primary organizational principles, such as that of continuity, similarity, closure, or formal field constructs, such as that of stationary equilibrium, and apply them to complex motivational forms of individual and social behavior; such "dynamization" of formal principles has been espoused for the analysis of variegated individual or social behavior by Duncker (1), Lewin (5), Krech and Crutchfield (3).

Some of us wish to start interpretation at still higher cognitive levels; the attempt is made here to understand the novel facts about perception by broad principles of cognition and recognition. The symposium papers by Wallach and by Bruner and Postman most impressively demonstrate the value of such a venture.

Finally, personological principles might be employed that may serve as the general interpretive framework within which simple or complex perceptual behavior is to be analyzed and understood; Klein's important paper, presenting a comprehensive program of research in perception, is directed toward this aim.

Considering the hierarchic structure of personality, within which perception occupies an intermediate position, it seems only logical that integrative interpretation should be ventured at various levels, "from above" as well as "from below." The over-all and gratifying feature of this symposium seems to be that its participants feel keenly about the necessity of bringing unity and system into a field to which all of them—the speakers as well as the discussants—have already contributed so signally.

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SOME CONSIDERATIONS CONCERNING THE RELATION BETWEEN PERCEPTION AND COGNITION

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I look around in a familiar environment and find that nearly every object has meaning. This meaning I experience as an objective fact, and I perceive it out there in the thing. A hammer looks like something with which to drive a nail into a wall or something with which to smash a vase. I have the impression that I perceive these meanings in the object even while I realize that they do not come to me through my eyes at the moment of perceiving them but must be furnished by a memory function, for they were given by previous experience with the object.

I suspect that this discrepancy between experience and function is responsible for some of the vagueness of conception in this part of psychology. The remedy seems to be to acknowledge it explicitly: The meaning which the hammer has for me is, *functionally* speaking, the effect of past commerce with that object, but it is experienced as being seen in the object of equal status with color and form. We have here the effect of a recall process in which recall is not experienced as such. Instead the contribution of memory appears as part of a percept.

There is no reason why this recall process should be essentially different from other recall processes as, for instance, recognition and recall by association. As Köhler reviving an old argument of Hoeffding's has pointed out (5; 3, pp. 126-144), any recall which is occasioned by a perceptual experience involves a process in which such an experience brings into function a memory trace of a similar experience of the past. This is true also where the subject recalls an associated content. When I pass a man on the street whom I have met before, I may, for instance, recall his name. This recall presupposes an association between a visual trace, say of the man's

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[1] This paper was written while the author was a John Simon Guggenheim Memorial Fellow.

face, and a trace of his name. But the association alone does not explain recall of the name on this occasion. Face and name left associated traces when I was introduced to the man in the past. When I meet the man again, he is, of course, on the whole the same physical object, and I may immediately experience him as the same person I met before; but *functionally* speaking there are two separate psychological events involved, the perception of the face on the occasion of the first meeting and a present perceptual process of the "same" face. The former is now represented by a memory trace, and it is this trace which provides the access to the trace of the name. Yet it is one of many traces of faces which I might recognize, and so the question arises how the appropriate one is brought into function. As Köhler has argued, the only possible answer is that the similarity between the original process of seeing the face, now represented by its trace, and the present process is responsible for the proper selection of this trace. Recall by association, then, consists of two steps: A process of recall by similarity by which the present perceptual process makes contact with the trace of a similar process of the past, and secondly, recall of a content associated with this trace.

The necessity for such an assumption is not avoided by thinking in terms of responses instead of associated contents. One speaks of the same stimulus when the same combination of physical processes affect the sense organs of the subject or, more loosely, when these combinations appear to the experimenter as the same. Yet, when the same stimulus occurs repeatedly to produce a particular response, we deal, of course, with different, initially unconnected psychological events, and they must in some fashion manifest their identity in the nervous system of the subject before that particular response can be aroused. In a relatively small number of cases the assumption might possibly be made that afferent processes produced by different stimuli will arrive at different places in the nervous system and manifest the identity of the respective stimuli by their locale. But where the stimulus is characterized only by its spatially or temporally extended pattern as, for instance, in the case of visual forms, of speech sounds, and of noises, this assumption cannot be made. Similarity between the processes produced by different occurrences of the "same" stimulus must account for its identification.

Recall by similarity may occur without recall of an associated content, namely, in pure recognition where it merely produces a feeling of familiarity.

The same process of recall by similarity must be involved when meaning is perceived, unless a set is operating. No matter what the nature of meaning may be, as long as a meaning content was acquired in the past it is necessary to postulate the same sequence of recall processes in order to explain how meaning comes to appear in perceptual experience: first, a step of recall by similarity between the sensorily determined perceptual process and a trace complex with which the meaning content is connected, and secondly, the coming into function of the connected meaning. Since in a familiar environment most perceptual objects appear meaningful, such recall processes must readily take place all the time, and because they do, their mediation usually goes unrecognized.

I have just used the term trace complex, because often what appears as a familiar or meaningful object has occurred in the past not only once but at several different times. Frequently a number of these occurrences have contributed in different ways to the meaning content. When recall takes place, the total meaning seems to be given. This indicates that the recall process established contact, not with a trace of any one of these occurrences, but with a trace complex which represents features of a number of them. In short, the various occurrences must have previously become connected so that they could contribute to the total meaning content and later, in a recall process, function together. These connections are themselves the effects of recall processes which took place in the past, when at the second occurrence of the object contact was made with the trace of the first one, and later, when the third occurrence brought into function the trace complex of the first two, and so on. In this fashion, recall by similarity accounts for a cognitive product of temporally separated events and, in many cases, makes learning by repetition possible.

Often the function of a memory trace which participates in a perceptual process is not merely to add its content to the sensorily determined qualities of color and shape. It may change the organization of the primary process by imposing a different internal grouping as in the case of the Street Figures. Or new perceptual qualities may emerge after a trace reference has been established, as, for instance, physiognomic qualities. They are seen only when the primary form pattern has been recognized as a face. But when that has happened, the seen physiognomic qualities depend mainly on the sensory conditions.

Yet no matter how intimate and varied in function the interplay of a primary process and an aroused trace may be, it must be preceded by a process of trace selection. Where no set is operating, this selection process consists of recall by similarity and is therefore initiated by the primary perceptual process and highly dependent on its characteristics. The selection process must be distinguished from the interplay between trace and primary process, for it is prerequisite for this interplay.

The intervening of this selection process before a pertinent memory trace can influence a perceptual process introduces an element of fortuity into experiments on the effect of central factors on perception, or, rather, what amounts to fortuity from the viewpoint of such studies. We are all aware of the difficulties which the incomplete knowledge of the subject's past means for experimentation and its evaluation. But even if we had a complete inventory of the subject's previous experience, we would not be much better off because it would still be to a degree fortuitous whether a psychologically pertinent trace content actually comes to participate in the present process. Köhler and Restorff have demonstrated that whether or not an experience of the past through recall by similarity gains an influence on a present process depends on the content and structure of the time interval between that experience and the present (**1**; **5**; **3**, pp. 126-144). Frequency and recency of the mental event which the pertinent trace represents undoubtedly also plays a role in whether or not contact is made with the trace (**1**, **5**). Therefore, where trace selection is left to spontaneous recall, the influence of a need or of another central factor on perception is not altogether a dynamic matter. This must be borne in mind when quantitative results are interpreted. They represent not only the strength of the central factor in the perceptual process but also the probability of the occurrence of the contact with the pertinent trace.

This last consideration presupposes, of course, that, as in the case of meaning, central factors gain whatever influence they may have on perception through mediation of a more or less specific memory trace. This is certainly true of values which with few exceptions are themselves mediated by meaning. This is also true of needs. On the whole, particularly in a mentally healthy individual, a need becomes operative only in specific situations. A given situation must be recognized as belonging to a certain kind which is specific for the operation of a particular need. It is even true of drives when they are aroused by their drive objects. Except for the relatively rare cases where cathexis of a novel drive object occurs in the perceptual situation, the capacity of an object or situation to arouse a drive was acquired in the past, and this product of previous cathexis is brought into function through a recognition process.

That the arousal of a pertinent memory trace must precede the influence of central factors on perception is particularly important where these factors are supposed to affect recognition, as in the case of tachistoscopic studies (6). It seems very likely that the arousal of a pertinent trace amounts to recognition. But the assumption that values and needs affect recognition seems to contradict this simple conception. For these central factors which supposedly have an influence on whether or not recognition occurs come into play through the arousal of pertinent traces. This makes no sense if recognition is equivalent to trace arousal. A solution to this dilemma will have to be found in a closer analysis of the psychological processes which go on in such experiments.

Up to the present the main interest of the studies on the influence of central factors on perception has been in demonstrating such effects. Such demonstrations show to what extent the organization of perceptual processes can be influenced by contents and conditions of central origin. But this is not the only way in which studies of this kind may contribute to the investigation of perceptual functions. They may be helpful in answering the following question: To what stage of organization must a visual process develop before it can arouse a memory trace? Since, in the absence of a "set," central factors gain an influence only *after* pertinent traces have been aroused, properly designed experiments which attempt to produce such an influence may be a way to answer this question.

I should like to clarify this question briefly: A number of perceptual functions besides the strictly sensory processes have to take effect to bring about visual percepts as we experience them. They are, to name only those which are of interest in this context, the interaction process on which the formation of dense surface colors depends, organization due to grouping factors, and the formation of "figure and ground." The question is whether they must also take effect before a pertinent memory trace can be aroused.

The answer to this question has certainly to be affirmative in so far as the colors of the achromatic scale are concerned. The gray color seen in a certain region of the visual field depends on the relation of the intensity of stimulation received from this region and the stimulus intensity of the surrounding region (9). An interaction between the local processes must be responsible for the gray color. A light gray can be changed into a dark gray either by lowering the stimulus intensity in the region in which the gray is seen or by raising the intensity in the surroundings. The end results of these changes cannot be told apart, although in one case stimulation in the gray region changes and in the other it does not. This shows that no sensory process which corresponds to the intensity of local stimulation has trace representation; only the product of the interaction process plays a role in recognition.

The evidence concerning grouping pertains only to line figures. Demonstrations of Köhler's (4) and the work of Gottschaldt (2) have made it clear that figures with which the subject is well acquainted will not be spontaneously recognized when lines are added which prevent these figures from appearing as separate units. I think it is easy to rule out the possibility that the comprehensive figure which comes about through such addition prevents recognition of the "hidden" figure by initiating its own recall process in competition with the "hidden" figure. Rather, "hidden" figures fail to be recognized because they do not appear as separate units and are therefore unable to make trace contacts. Since in the case of line figures the formation of units is ruled by grouping principles, organization due to grouping factors should precede trace arousal. There is certainly need for direct evidence to support this reasoning.

The evidence concerning "figure and ground" is at present contradictory. The essence of the distinction of figure and ground is that only the area designated as figure has form. Rubin has demonstrated that an area will be recognized by its shape only if it is seen as figure (7). This means that the sensory pattern must first be organized in terms of figure and ground, before the figure areas establish contact with specific memory traces.

This seems to be irreconcilable with the result of the brilliant experiment of Shafer and Murphy (8). It demonstrated an influence of previous reward and punishment on the perception of figure and ground in an ambiguous design. An outline circle was divided by an irregular vertical line of such a shape that each half of the circle could be seen as a profile of a face of half-moon shape. Prior to the test in which two such ambiguous figures were used each half-moon shape was presented singly, and each presentation was always accompanied by a reward or a punishment. In the test, the previously rewarded half-moon shapes were predominantly recognized, that is, seen as a figure.

There can be no doubt that in this experiment the aftereffect of previous reward and punishment was connected with the traces of the forms of the training series and could become effective only through the arousal of these traces. Thus, figure-ground distribution is influenced by factors which can only come into play *after* the pertinent trace has been aroused. This seems to contradict the conclusion drawn from Rubin's result that formation of figure and ground must precede trace arousal.

This contradiction disappears only when one takes into account that Rubin worked with solid figures, that is, pattern where the contour between figure and ground is formed by the borderline between different colors, whereas Shafer and Murphy used outline figures. The black line on white which here forms the contour between figure and ground also has form in its own right and can therefore make trace contact. A repetition of Shafer and Murphy's experiment with solid figures would really go to the heart of the matter. My hunch is that it would fail to demonstrate any influence of reward and punishment on figure-ground distribution.

I have recently become impressed with the extent to which memory traces participate in simple perceptual processes. They not only impart meaning or impose grouping as previously mentioned; they also seem to be responsible for the perception of three-dimensional form where perceptual conditions for primary organization in depth are absent or weak.

In this situation the full answer to the question, at what stage of perceptual organization pertinent traces can be aroused, seems to be very much needed. As matters stand I would say that the interaction process on which the formation of dense surfaces depends, the formation of figure and ground, and in some cases, organization due to the grouping factors must develop before traces can be aroused through similarity of form. That such processes must occur before traces can be aroused may possibly be one of the reasons for the great stability of simple perceptual processes and the high degree of independence from other psychological functions which they exhibit. One may say that, up to a certain point, the development of percepts is protected against interference by central factors, because the developing percepts are inaccessible before trace contact has been made.

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PERCEPTION, COGNITION, AND BEHAVIOR

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The study of perception during the past decade has been moving along two converging paths. Our symposium today is an effort to discover whether the two paths have yet converged or, if not, whether some kind of bulldozer operation can be set in motion to widen the two paths into a single road.

One approach to perception is primarily concerned with perception in and of itself, without primary regard for the manner in which perception is imbedded in the other, ongoing activities of the perceiver. There is relatively little concern with the role of perception in the adjustment of the organism. The functional relations between the dimensions of the stimulus and the attributes of experience and the determination of these relations by sensory mechanisms concern some exponents of this approach. Others stress the intrinsic laws of perceptual organization (e.g., the law of prägnanz). In research of this kind it is crucial to control or to hold constant the set, motivation, and past learning of the subjects. The subject is instructed to be neutral, accurate, and attentive. He is assumed to have a basically neutral attitude toward the stimulus, wishing neither to eat, destroy, caress, nor in any manner to use the stimulus presented to him. It is merely a something to be seen, heard, touched, smelled, or sensed. Sometimes, it is true, the parameters of set and past experience are systematically varied and related to changes in perceptual response. But even when they are considered, such factors are assigned a distinctly secondary place, as for example in Wertheimer's laws of perceptual grouping (40). They are used to account for residual variance. In general, to use Brunswik's language, stimulus variables and organism variables remain artificially tied in a "standard" experimental situation, and no attempt is made to obtain truly representative examples of both sets of determinants (9). Let us label the practitioners of this point of view. They are Formalists.

The second approach attempts to treat perception in a broader behavioral context. For its primary concern is with the manner in which perceptual functioning is imbedded in and interacts with other forms of psychological functioning. Perception is viewed as instrumental activity. Those who take this approach to perception hope to add to and to go beyond the variables and relationships discovered in the formal study of perception, not in any sense to disregard them. For a full understanding of the perceptual process it is necessary to vary not only the physical stimulus and the sensory state of the organism but also those central conditions—motives, predispositions, past learnings—which have largely remained outside the formal limits of the perceptual system. The data of such research—the dependent variables—are often the same as those of the Formalists: sensory discriminations and characteristics of perceptual organization. A wider universe of determinants related to the activities of the organism is sampled. The goal is a "full information" theory of perceptual behavior. The students attracted to such research earn a label too. They are Functionalists.

That the two approaches are eventually convergent is obvious. Formalists and Functionalists can and do live fruitfully in the same laboratory. Figure-ground organization, a primary datum of perceptual dynamics, has been shown to be a function not only of brightness differences, relative size, and clearness of contours but also of satisfactions and deprivations associated with some figures and not with others (36). Is it inconceivable to think of families of curves relating stability of figure-ground organization to such factors as brightness difference or relative size, with degree of satisfaction (independently defined) as a parameter?

Our task, of course, is to investigate the kinds of mechanisms which mediate the effects of such central processes as satisfactions and deprivations on the operation of perceptual processes and to reach some understanding of how these changes serve in the overall economy of the organism.

OBJECTIVES

Perhaps the most serious block in our progress to convergence has been a lack of systematic rigor amongst the Functionalists concerning the variables which they are required to study by the nature of their theories of behavior. What do we want to vary by way of central or directive states in the organism, and what kinds of predictions do we want to risk concerning the effects of such variation on what kinds of perceptual processes? We have reached the point where there have been enough experiments which have as their conclusion the simple demonstration that need or past experiences influence one or another perceptual performance. Our task for the future can be stated, we think, in terms of the following objectives, all to be pursued simultaneously.

(1) To select central, nonperceptual variables, changes in which can be shown to bring about systematic changes in perceptual functioning. (2) To select these variables from various theoretical systems—learning theories, motivational theories, theories of personality—so that these theories may be made continuous with the body of perceptual theory. (3) To postulate and then study those *intervening mechanisms* which account for the changes in perception which occur when we change the central state of the organism. (4) Finally, to emerge with a unified theory of behavior which treats the organism as an organized whole and which contains laws stating the manner in which perceiving is an instrument of adjustive activity.

The present paper is an attempt to deal with two of the four objectives stated above.

The Theoretical Status of Directive Variables

The Functionalists are faced with choosing basic cognitive and motivational variables which are systematically related with perceptual functioning. In the interest of increasing generality and comprehensiveness of our theories of perception, these variables must be related to fundamental principles and postulates of contemporary theories of behavior.

Theories of conditioning and learning. The past decade has furnished many examples of research in which variables relating to different theoretical systems have been brought to bear on perception. Learning theory is a case in point. We may distinguish three broad types of approaches toward the integration of perception into general theories of conditioning and learning.

One approach has been to *treat perception as response* (e.g., a form of verbal response) and to investigate whether it follows the laws of response derived from research in other forms of behavior (primarily the acquisition and performance of motor responses). Some years ago, Ellson (14, 15, 16), for example, attempted to show the manner in which frequent pairing of a signal and a sound affected the formation of a conditioned auditory hallucination. He noted that classical principles of conditioning and extinction do not govern the phenomenon. Using less tangential perceptual phenomena, Solomon and Howes (37), treating perception of verbal stimuli as an instrumental response, have attempted with some success to relate speed of perceptual recognition to frequency of prior (reinforced) usage of this response.

Another approach, stemming from conditioning theory, attempts to account for perceptual organization and changes in organization by invoking intervening mechanisms derived from conditioning principles. Hull's "afferent neural interaction" is an example. It is through afferent neural interaction that the kind of complex perceptual response which is studied by Gestalt psychology is to be subsumed under the general principles of Hull's behavior theory. The work of Lambert, Solomon, and Watson (23) indicating that the apparent size of objects varies as a function of the amount of reinforcement associated with them is another instance.

Finally, there is the kind of conditioning research—perhaps it is not fully correct to subsume it under this category—which attempts to *infer* perceptual or cognitive organization from responses observed in the learning situation. Zener's work is almost a pure case of this (42). But, generally, such research—including Tolman's cognitive maps (38) and Hilgard's "broad functionalism" (20)—is less a matter of "conditioning theory" or "learning theory" than an approach to a general theory of cognition in which perceptual organization and reorganization mediate behavioral change.

Gestalt theory. Gestalt theory, for all its stress on "pure" perceptual laws, has been aware of central directive determinants which cannot be predicted from the stress of the perceptual field toward optimal organization. To encompass such phenomena we must invoke the interaction of stimulus processes and memory traces. In this view, so well espoused by Wallach's paper (39), central directive processes become effective when (a) there is a change brought about in the trace field by the central (e.g., motivational or experimental) state investigated, and (b) when there are adequate conditions for communication between stimulus process and trace. Duncker's work on color constancy (13), the experiments of Djang (12) and Braly (3) and Henle (19) on the effects of past experience which refined Gottschaldt's work on the same problem (17), are instances of research generated by this conception. In most instances these studies get little beyond the scope of Formalist research, their emphasis being upon the formal properties of the stimulus process and the trace. It is interesting that Koffka's conception of the ego (22) had so little influence on this phase of Gestalt research.

Motivational dynamics. Another theoretical approach toward the study of perception in broader behavioral context stems from theories predominantly concerned with motivational dynamics. Generally, these theories are only minimally concerned with the mechanisms which mediate between motivation and perception. Their interest is in the manner in which perception serves motivational requirements. Perceiving is here regarded as regulated by the economy of personality. It operates to aid in wish-fulfillment, in the reduction of tension, in reality testing, in ego defense, etc. One may make predictions about the selectivity of perception based upon psychoanalytic principles and, indeed, make predictions about differential and absolute sensitivity. But Freudian theory tells us nothing about the mechanisms which bring about such perceptual changes. Yet, either directly or indirectly, there have been inspired by the Freudian view of personality such provocative studies as those by Murphy and his students (24, 32, 36) on perceptual autism; by McClelland and his collaborators (1, 26) on need gratification; by Sanford (34, 35) on the same topic; by Klein on threshold phenomena (21); and by other authors on a host of other topics mostly clinical in nature. Our own work, too, has been indirectly influenced, as have the continuing studies of Witkin and Asch (41) on personality determinants of orientation in space. Yet few of these studies have sufficiently concerned themselves with intervening mechanisms—although it may be argued that all of them may be handled by Gestalt trace theory, for example, or by conditioning principles.

Next steps in research might well attempt to arrive at a formulation which is equally concerned with the instrumental nature of perception, with the role of central need states and past learning, and with the mechanisms that intervene between central factors and perception.

Intervening Mechanisms in Attributive Perception

The remainder of this paper is concerned with an exercise in formulating intervening mechanisms which can account for some of our own recent results and the results of other investigators. It is an exercise which illustrates the problems involved in gambling on intervening mechanisms.

Our own research has been limited to two general types of perceptual phenomena. The one may be called attributive perception: studies of judgments of size, movement, brightness, and similar abstracted perceptual attributes. To date, our concern in this area has been mostly with the extent and direction of systematic judgment tendencies—with a sampling of conditions of set and motivation under which objects are judged as bigger or smaller, faster or slower, brighter or duller, taller or shorter. A second series of experiments deals with the speed and course of the recognition process under varying directive states.

Determinants of attributive change. The first group of experiments, concerned with the attributive characteristics of objects of different personal relevance to the perceiver, raises this problem: Is there a tendency for that which is valued to be vivified or perceptually *accentuated* and, if so, in what manner and under what conditions? An early study by Bruner and Goodman (5) seemed to indicate that, within limits which we must discuss, the more valuable a coin, the more would its size be overestimated by ten-year-old children. Lambert, Solomon, and Watson (23) have improved on the technique of this experiment by setting up a token-reward situation, demonstrating that after a poker chip has served as a token which can obtain a reward, it comes to be judged larger than when it was simply a neutral object. Experiments conducted by Bruner and Postman (6) and repeated with modifications by Bragman (4) indicate that tokens containing a positive symbol are judged larger than those containing a negative one, and both are judged larger than one containing a neutral design. But, as Klein (21) has shown, there are situations (perhaps ones which are "accuracy oriented") in which this effect is washed out. Indeed, as these experiments continue, it becomes apparent that under certain conditions size accentuation is a function not of *positive* value as such but of *degree of personal relevance.*

A recent experiment by Postman and Bruner (30) serves to underline the point. Young children were set the task of judging the height of standard three-inch toy figures. The figures, intrinsically desirable, were overestimated in height as compared with control blocks. Children were given the toys to keep during the first part of the experiment and were promised that they would continue to receive them. When the experimenter broke his promise, when the toys became unavailable, their apparent size underwent a further increase. A control group showed no comparable increase. Frustration in the situation, the unavailability of the desired object, served to provide another jolt in the personal relevance of the objects. Increase in apparent size resulted.

All the situations thus described are, however, ones in which the manipulation of the object being judged is not crucial to the behavior being called for. It is under such conditions that changes in attributive judgment are most likely to occur. An experiment (7) illustrates the point. If one increases the relevance of an object by imbedding it in a situation where it must be manipulated accurately if the subject is to minimize punishment, size judgments of the object tend toward maximum accuracy.

The problems involved in accentuation as a function of personal relevance need considerably more investigation. The very term "personal relevance" is itself somewhat difficult to render into operational terms. Yet the fact of accentuation provides an important challenge to research. Under what conditions does accentuation occur? Carter and Schooler (11) have found, for example, that their subjects do not show perceptual accentuation as a function of the increasing value of coins. Their experiment utilized a perfectly round variable figure which their subjects adjusted to the size of the coins. Bruner and Goodman used a variable figure made up of nine elliptical sides. They obtained the effect. When Carter asked his young subjects to reproduce the sizes of the coins without the coins being present, he too obtained overestimation varying with value. What kinds of constraints operate in the stimulus field which enhance or inhibit the operation of directive factors? And what kinds of instructional or motivational constraints operate? Does a "critical" or "analytic" attitude, for example, serve to cut down the impact of other motivational factors? Until we know the answers to these questions, inconsistency of results in experiments using different instructions will be the rule.

Intervening mechanisms. Can the results of these experiments be understood by reference to a plausible intervening mechanism? At the descriptive level, we have subsumed these phenomena under the heading of *accentuation.* We have conceived of accentuation as a central process which leads to systematic tendencies in attributive judgment, increased saliency of the personally relevant. Accentuation can, of course, be a useful intervening variable only to the extent to which it is anchored to antecedent and consequent conditions. The consequent conditions are attributive judgments—judgments of size, brilliance, speed of movement, etc. The antecedent conditions have been sampled by the experiments which we described: the range of values of the attributes which the subject has experienced in the past; frequency and amount of reinforcement associated with the object; or the availability of the object for manipulation and consumption in relation to the subject's need for the object. When anchored to such antecedent conditions, accentuation becomes more than a class name for a group of judgmental "distortions." It becomes an intervening variable which can lead to limited predictions about attributive judgments.

Our understanding of accentuation is further aided if we can show it to be a special case of more general or inclusive mechanisms mediating attributive judgment. We believe degree of accentuation to be a special case of what Helson (18) calls *adaptation level.*

Let us assume first that the value of any attribute—size, hue, speed, etc.—is not an invariant function of stimulus magnitude. In the presence of a given physical extent, for example, apparent size will vary over a certain range under a sampling of conditions (10). At any given time, one of the principal determinants of the perceived value of an attribute, as Helson has insisted, is the adaptation level of the organism with respect to the attribute in question. The apparent size or speed of a given object to be judged is a function of its distance from the neutral point or adaptation level of the range of such stimuli which the person *has* judged. Adaptation level, in short, is the organism's *subjective probability ("expectation") about the middle of the range* of sizes (weights, speeds, etc.) which belong in the same class as the object being judged. In a series of judgments, Helson has shown that "adaptation level can be closely approximated by assuming that it is the weighted geometric mean of all stimuli being judged" (**18**, p. 302). Let us take an example of its operation. Duncker (**13**) performed an experiment in which a leaf, cut from green felt, was presented to his subjects in concealed red illumination. The subject made a color match. Then a donkey, cut from the identical cloth, was presented in the identical illumination. Another color match was required. His results show that the leaf appeared greener than did the donkey. Under these circumstances, the stimulus is assimilated to the adaptation level for the color of a green leaf—an adaptation level built up in the course of commerce with green leaves in the springtimes of the organism's past history.

The phenomena of accentuation—take size for example—suggest that under certain conditions the adaptation level for valuable, important, personally relevant objects is higher than for "neutral" objects. Adaptation level is determined by the *traces or aftereffects* of the stimuli with which the organism has had commerce. So too memory. Now there is experimental evidence that objects which are important or personally relevant are remembered as larger than neutral objects (**5, 11**).[1] The trace system is characterized by saliency of the valuable and personally relevant, which is the same as saying that valued objects have a higher adaptation level. In attributive judgment of a valued object, the stimulus is assimilated to this high adaptation level. In consequence it appears large and vivid.

Our discussion shows that accentuation, adaptation level, and memory change are closely interrelated, that translations can be made from one system of conceptualization to the other. Such translation leads, of course, to a search for common principles manifesting themselves at these different levels of analysis. We must still face the basic question: Why is the trace system characterized by greater saliency of the personally relevant (which implies systematic memory change, higher adaptation level, and accentuation)?

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[1] We may well inquire if it is a universal thing to find the good and important associated with largeness, or whether this is characteristic of our culture. Certainly our language is full of size-value linkages: a successful man "grows in stature," an important problem "looms large," etc. We even have moral dicta warning us against the linkage: "Good things come in small packages"—a maxim not readily taught the young child.

The phenomenon we have described as "accentuation," then, may well be considered as a special instance of the determination of judgment by adaptation level. Such a translation is fruitful: it may sharpen the precision with which accentuation effects can be predicted, using the quantitative theory of adaptation which is already in the process of development. At the same time, the dependence of adaptation level on central directive states of the organism is brought into clear focus.

Recognition Phenomena

Several of our own experiments and several experiments by other investigators have raised problems concerning the manner in which perceptual recognition is related to motivational and cognitive states of the organism. Again, let us review briefly some of the salient results before searching for mechanisms.

Determinants of recognition. One series of experiments indicates that perceptual recognition, defined as *correct report*, is the more rapid the more the stimuli used are familiar, probable, or congruous with prevailing attitudes, values, or needs. That prevailing state may be a somatic or social need as in McClelland's work (1, 16), a dominant value as in the work of Rodrigues (33) or of Postman, Bruner, and McGinnies (31), or of Blake and Vanderplas (2), a strong response tendency built up by frequency of contact with the stimulus as in the work of Solomon and Howes (37), or expectancy as established by the sheer probability of the occurrence of certain kinds of stimuli as in an experiment of Miller, Postman, and Bruner (29). When a stimulus "fits" or is appropriate to the prevailing state, it is recognized more readily (29). That which is incongruent with the prevailing state of the organism is recognized more slowly and with greater difficulty. All of which is not to say that recognition is always wishful, for it is apparent that all prevailing states of the organism are not instances of primitive desire for pleasure. We seek information to aid in adjusting as eagerly and as often as we seek simply wish fulfillment. It is for this reason that we avoid such expressions as "autism" (36) to characterize the phenomenon under discussion.

We have attempted to investigate the specific ways in which congruity or incongruity with the prevailing state of the organism affects the recognition process (8). Subjects, for example, are shown tachistoscopically at increasing exposure a series of playing cards. Some of the playing cards are "normal," some are incongruous with the organism's subjective probabilities in the sense that color and suit are reversed. That is, in addition to regular cards, subjects are shown such monstrosities as a red six of clubs or a black three of diamonds. As might be expected, there is a very large and significant difference in the exposure time required for correct recognition of the two kinds of playing cards, the incongruous one requiring a great deal more time.

What is particularly interesting is the manner in which the organism attempts recognition in the presence of inappropriate stimulation. One mechanism which leads to quick recognition of the normal cards and which operates against correct recognition of the incongruous ones is what we have called "dominance." It is a mechanism very much akin to assimilation. On the basis of partial recognition—say, recognition of suit alone or color alone—complete recognition is attempted in accordance with past experience. When the card is, indeed, normal, the attempt is successful. When the stimulus contains an incongruity, errors in recognition result. The incongruous cards are seen as normal: the red six of clubs will be reported as the black six of clubs or as the red six of hearts or diamonds. Color will be assimilated to form or form to color. One determinant will be dominant, the other assimilated to it. In the presence of incongruous stimuli, a second reaction frequently occurs: a compromise formation (perhaps partial assimilation) in which the incongruous card will be seen as midway between the value of the stimulus and the "value" of the expectation: a normally black suit printed in red, for example, will be reported as purple, darkish red, brown, black in red light, red in shadow, etc.

The same phenomena at a more complex level can be observed when more "meaningful" or emotionally *lebensnahe* stimuli are used. Present tachistoscopically a picture of a love scene, a handsome young Arab leaning yearningly over his beloved on a couch in a Moorish setting. In the incongruous version of the stimulus, there is substituted for the "normal" and lovely beloved, an ugly old hag.

Again there are dominance and compromise reactions at the lower exposure levels: the hag is seen as a young woman, or the two characters are seen as middle aged, or the scene is made congruent with "norms" and expectations in some other ingenious fashion.

We come finally to a set of phenomena which are of central importance in a wide variety of behavioral situations—normal and pathological. Briefly, the facts are these: when a subject is presented stimuli which are known to be threatening or otherwise anxiety-provoking, he may misperceive the stimulus on tachistoscopic exposures preceding correct recognition in one of several characteristic ways. First, he may fail to perceive anything at all, a kind of perceptual blocking. This may occur at exposure levels above his "normal" threshold. Or he may report seeing stimuli which are jumbled and nonsensical or incomplete. Finally, he may report seeing material which is either derogatory or contradictory to the nature of the actual stimulus. These phenomena suggest to the guileless investigator the image of the superego peering through a Judas eye, scanning incoming percepts in order to decide which shall be permitted into consciousness.

What requires explanation in terms of intervening mechanisms in the experiments mentioned are these facts: (a) that stimuli which are congruent with the prevailing directive state of the organism are more readily recognized than incongruent material, (b) that incongruent stimuli are "distorted" to conform to the dominant need or expectation of the perceiver. In short, the facts can be summed up by saying that perceptual mechanisms operate to select and maintain those aspects of the stimulus habitat which are in keeping with or congruent with the prevailing state of the organism—whatever that state may be.

Intervening mechanisms. What perceptual and cognitive mechanisms can be invoked? Let us assume first that recognition requires some process of communication between an incoming stimulus and a trace. One can restate the matter in stimulus-response terms by saying that an incoming stimulus, in order to yield a recognition response, must mobilize a connection with some response mechanism. One need not make a final commitment concerning the two views. For our purposes, they represent alternative ways of stating the same problem. Our next assumption, again a reasonable one, is that all traces or all responses are not equally available to mobilization by any given stimulus process. Certain traces or responses whose activation has led to punishment or deprivation in the past may be highly unavailable. Or unavailability may be a function of sheer disuse or of failure of prior reinforcement. Other traces or classes of traces (or responses, if you will) are highly available to mobilization by the stimuli. Indeed, they may be not only highly available but indiscriminately so in the sense of being available to a broad range of stimuli, i.e., there is a broad gradient of stimulus generalization.

A final step in our account is the assumption that different overall need states of the organism or different states of expectancy bring about characteristic changes in the differential availability of different traces or, again if you will, in the strength of different response tendencies. In short, ease or difficulty of recognition is a function of changes in availability of trace or response systems to incoming stimulation.[2] To be a useful intervening mechanism, availability must, of course, be anchored to specific antecedent conditions such as frequency of reinforcement as well as specific motivational conditions at the time of the experiment.

It is this very same differential availability which leads to certain characteristic "normalizing" prerecognition responses in our incongruity experiments. As the subject begins to recognize the generic characteristics of the stimulus—prior to full and correct recognition, when his only knowledge, say, is that the stimulus is some kind of playing card—that partial information activates traces (responses) appropriate to playing cards of the sort with which the person has had commerce in the past. These become available and prior to correct recognition are crucial in determining the kinds of "dominance" responses which we have described above.

What about the "defensive" perceptions which we have described, by which the subject frequently negates the nature of the actual stimulus in his report? At first blush we seem to be facing a paradox here. In order to repress or negate a stimulus, must the subject not recognize it first for what it is? The paradox remains only so long as we (1) restrict the definition of *recognition* to one type of response—veridical report and (2) insist that all systematic responses to the stimulus depend on the *prior* occurrence of correct recognition. Neither of these restrictive assumptions is necessary. There can be tripped off, by the presentation of a stimulus, a multiplicity of response tendencies among which veridical reporting is only one, albeit a most important one. Other systematic reaction tendencies tripped off by the stimulus may be largely affective in nature and lead to various forms of avoidance responses. Each of these possible responses has its own threshold, determined by characteristics of the stimulus and by the directive state of the organism (differential "availability" of the responses). Thus, veridical report has *its* threshold, and affective avoidance response has *its*. As our experiments suggest, the threshold for affective avoidance response is frequently lower than the threshold for veridical report, but there is no need to assume that this sequence is invariant any more than we need to assume that some implicit "correct recognition" must take place prior to an affective response. In each situation there is a *hierarchy of response thresholds,* but the hierarchical sequence is by no means fixed.

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[2] An important parallel can be drawn to the distinction which is frequently made in learning theory between the acquisition and utilization of habits. Mobilization of a habit depends on the presence of an appropriate drive condition. Similarly mobilization of a perceptual response depends on the need state of the organism.

Even within the class of responses which we may describe as veridical reporting there is a variety of specific response tendencies each with a threshold of its own. This hierarchy of response thresholds results in what classical investigators have called the "stages of perception." Observing the behavior of a subject in a tachistoscopic experiment, it is quite apparent that there are *many* thresholds along the way to correct recognition. A subject shown a horse may report first that it is a "smudge," then a "thing," then "some kind of animal or person," then that it is an "animal of some kind with four legs," and then finally that it is a horse. In short, rather than thinking in terms of "an absolute recognition threshold," it is closer to the facts to posit a hierarchy of thresholds, each requiring a different quantity and, perhaps, a different configuration of stimulation before a response is tripped off.

If this rather reasonable assumption of the hierarchy of thresholds be accepted, a Judas eye notion of double-perception is not required to account for the phenomena of "defensive" perception in which avoidance responses seem to precede correct recognition.

Let us assume that in these cases prior to correct, conscious, reportable recognition of a specific stimulus, there is a more generic type of response appropriate to the class or classes to which the stimulus belongs. To put it in trace terms, classes of traces or trace columns are activated before there is differential activation of a specific trace. Among the generic responses tripped off at exposure levels below that required for specific recognition is a class of rather diffuse affective responses.

Suppose further that the stimulus trips off a class of *negative* affective responses or sets up a negative affective state. That response or state (a) may have the effect of preventing the operation of perceptual closure and thereby hindering recognition, (b) the same inhibitory state may account for the greater prevalence of lack of response, disorganized responses, and nonsense responses prior to correct recognition of negative stimuli, and (c) the negative affective state may activate and increase the availability of those traces or associations which are defensive, derogatory, or contravaluent in nature.

The model we have set up, admittedly speculative, suggests some experiments. One may argue, for example, that if affective responses are tripped off before there is veridical recognition of the potentially affect-laden stimulus, then one should be able to locate these affective disturbances independently of their effects on prerecognition behavior. McGinnies (27) has attempted to do this by a rather ingenious technique, and although his experiment will require replication under many different conditions, the results are suggestive. As stimuli he used words known to be emotionally toned—they were such socially tabooed words as *whore* and *penis*—and comparable words neutral in nature. These he presented to his subjects at exposure levels below the report threshold. At each exposure, prior to recognition, the subject's galvanic skin response was recorded, and he was asked to report what he had seen. McGinnies reports that there was a significantly greater GSR in response to the "unrecognized" emotional words than to the "unrecognized" neutral ones. The same type of experiment has been performed by McCleary and Lazarus (25) utilizing nonsense syllables paired with shock in comparison with nonsense syllables not so paired as a means of handling the thorny problem of the withholding of response by the subject. Again, prior to recognition their subjects showed a reliably greater tendency to galvanic skin reaction in response to the "unrecognized" words paired with shock.

Some Conclusions

It is, of course, far too early in terms of research findings to be able to say anything very firm or rigorous about the kinds of psychological mechanisms which mediate between the directive states of the organism and changes in perceptual response. This paper has been for us an adventure in trying to close a gap between such concepts as need or value on the one hand and such more specifically perceptual mechanisms as closure, assimilation, adaptive level, and the trace-process communication network. As far as we are concerned, it is almost impossible to indulge in too much speculation at this point. Tolerance is required: the great task for perceptual research is to locate the bridges between those behavior systems which describe the general directive states of the organism and their changes and the systems which attempt to account for perceptual functioning.

It will be argued, perhaps, that our account of perception is *too* functionalist in nature—that we are too given, for example, to describing even prägnanz (which may possibly account for some of our playing card results) as a form of defensive activity. Perhaps there is a reason for our extreme functionalism. For too long, we feel, there has been an exclusive concern with the isomorphism of brain fields and the structure of experience. The kind of functional analysis which we are proposing, the question which inquires what perceiving does for the organism, cannot accept the analysis of the behavioral fields and its isomorphism with the brain field as the end-all of psychology. As we have thought about the matter, it becomes increasingly clear that a thoroughgoing psychology of perception imbedded in a thoroughgoing general psychology must inquire into *all* the conditions—formal as well as functional— which determine how the person comes to represent the geographical field in the behavioral field, how, in short, the organism by perceiving comes to adapt to the external, distal stimulus.

Finally, we think that the time has come in the study of directive factors in perception when we must go beyond the *ad hoc* stage and make increasing linkages with other areas of behavior theory. This will involve the expression of aesthetic and theoretical preferences. Perceptual theory of the kind we have been talking about may become as badly riven tomorrow as motivation and learning theories are riven today—for these are the kinds of theory with which work in perception must make contact. We close on the rueful note that perhaps ten years hence there will appear in some prominent psychological periodical an article entitled, "There Are Six Kinds of Perception." But such multiplicity is preferable to a schizoid separation between perception on the one hand and everything else on the other.

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WHERE IS THE PERCEIVER IN PERCEPTUAL THEORY?*

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I

Few of us regard the perceptual system as self-contained and autonomous. A growing body of research gives respectful recognition to Stern's maxim, *Keine Gestalt ohne Gestalter*. But this is often only face recognition: In the careful research on seeing, hearing, feeling, the see-er, the hearer, the feeler are somehow obscured or lost. The person himself, the pivot of all these sensations, the source and root of motives whose influences are sought, is ignored as a determinant of his own perceptual behavior.

We feel his absence even more keenly when we are confronted by the personality theorist who is aware of the vital place that perception holds in the person's adaptive economy. He might ask, "If the personality and perceptual organizations are as intimately related as they seem to *me*, then in what way can you help us to understand the complexities of personality? To what extent are your laws of perception, laws of perceivers?" These questions of course are also pertinent to those who specialize in motor behavior, cognition, or learning. Personality theory, far from being an independent island with its own isolated structure, is really deeply imbedded in the data of all these fields. *In the last analysis the organization by the person of these systems of response may be what we actually mean by personality.*

If then a theory of perception would respect the integration of the person, it must deal with the integration of perception, for we assume that each response-system, including perception, represents the person's unity. Perceptual organization in this view is the counterpart of personality organization, and our higher-order constructs must respect this principle. Our argument will develop through several stages; first, we will show the consequences of ig-

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noring personality *structure* in perceptual research which ostensibly is personality oriented; second, we will propose a tentative scheme which could remedy this omission; and third, we will indicate briefly some methodological consequences of this new scheme.

Obviously, we consider to be quite incomplete that viewpoint in research which assumes sensory processes to be so completely stable, so autochthonous, that they are little influenced by the *kind* of person in which they are studied. Studies in this train of thought seek to funnel stimulus conditions and response possibilities to a fine pinpoint where subject-variance is negligible. Controls are rigid, often elaborate, and permit us to peek at only a single level of determination, usually the peripheral one. Since only a single, "universal" response property, in a single organ, interests the experimenter, he feels it to be irrelevant whether he uses one, two, or three subjects. (When he uses five, one wonders if his faith in his experimental design wavers.) Individual differences are embarrassing bacteria in this pure atmosphere of precise measurement. And what is generally overlooked is that this type of precision has just the effect of *preventing* central determinants of behavior from appearing. An example of this was the design of visual acuity studies built around the duplicity hypothesis, in which only a single level—retinal determinants—was allowed into the experimental spotlight. When positive results were obtained, it seemed not out of line to say that visual acuity is *purely* and *solely* a retinal matter. But this was a logical inference from the result, and an inevitable one only because of the methods used and the assumption guiding these methods. Note, however, that the conception is gaining support among workers even in this area that any perceptual phenomenon, even visual acuity, can reflect successive levels of integration within the person (20). The literature on psychosomatic factors in visual disorders and recent disclosures of hitherto unknown facets of autonomic regulation of peripheral processes also testify to this point, the latter opening up fascinating questions concerning the extent and nature of emotional and temperamental involvements in so-called "peripheral" phenomena (8, 11). Thus, even in the germ-free realm of sensory measurement it is possible that a personality theory may find data which will enrich it.

When we turn to perceptual studies in which the focus is ostensibly on personality, we find them dominated by motivation theory (2, 21, 24, 26). At first glance this research seems certainly to involve more and more of the person. But its main concern is with *part-relationships* rather than entireties, with *generalized states* rather than diversities. We learn how hunger affects perception, hunger defined as so many hours of food deprivation, and we seem to inherit a push-button system of generalized, part-relationships. Press the button "hunger," and out comes a perceptual effect. The person becomes a passive medium of his drives whose course bears nothing of his personal stamp. We also learn that values and needs can, but do not *always,* affect perception. Here again, without knowing the personal significance of these we cannot apply any generalizations from group tendencies to the single case. Certainly such group tendencies convey *something* of the person's substance but nothing of his essence. What they neglect is the interaction of a value or need-stimulus with an ego structure which must cope with it. It is too simple to say that motivation in the sense of goal-seeking under the pressure of needs or tensions is the *only* directive force in perception. It is more to the point to stress the ground-structure which gives a drive or value a special fate and quality in each person. The stimuli must inevitably interweave with such a fixed structure, and the percept is the child of the interaction. Whether or how a drive or value-stimulus will affect perception will vary with different outcomes of the interaction—with typical "solutions" to this adaptive task.

One of our experiments supports this conclusion (15). We used as our props a variation of an experimental design devised by Bruner and Postman (3). Refugees from Nazism were asked to make size-estimates of discs containing various symbols, including a swastika. Previously reported results led us to assume that the refugees' violent antipathy for the swastika would change the size of the circle. But it didn't. Actually the swastika had no greater effect than any less valued symbol. A crucial factor to note in interpreting this is that the value-symbols were fundamentally irrelevant to the task at hand—that of size-estimation. They were superimposed on the task and provided no aid to accurate judgment. Value was frankly expected to be disruptive, to have an error-producing effect. But this is not an inevitable effect of values or needs on perception; and, indeed, in this situation "reality-mindedness" would require that the symbols, like other irrelevant aspects of the situation, be ignored for more relevant cues. True, people differ in how easily they can ignore these symbols. But the locus of study then becomes, not the *situation,* but the *person* and how he is organized to cope with his values and needs.

Studies which approach the problem of personality and perception via the effects of *generalized* motivational states on *generalized* perception equate all responses of all subjects as if they had only a single meaning; thus they invoke a new variation of the old constancy hypothesis. This, so effectively rooted out by Gestalt psychologists on the level of theory, tends to creep in again and again through the back door of method. But when a phenomenological and person-oriented approach emphasizes the *understanding* of responses, *inequivalence* of the meaning of scores among subjects is more likely to be taken as the rule rather than the exception, and the interpretation of data becomes a quite different matter indeed.

Another experiment we have done recently on sensory thresholds in the hypnotic state (14) also demonstrates how insufficient are generalizations about group tendencies for describing the effects of personality on perception. One conclusion seems clear at this stage of the study: The *generalized* hypnotic state itself, inferred from the averaged data, may correspond to no consistent pattern of threshold changes. But the *individual subject* does show a reliable pattern of change under hypnosis. Of course such individual patterns must eventually be explained by a theory which can deal with the interlocked organizations of perception and personality.

But enough about current approaches which permit only part-relations and average tendencies to be discovered. We do not quarrel with the generalizations themselves that are made through such work since these may be altogether valid on one level. But we find it necessary to underscore their limitations for a person-oriented approach, and we must shift the axis of our quest. Our interest turns to hitherto neglected influences—the interweaving of personal expression into all functioning, which gives "togetherness" to perceiving. In what follows our deep debt to the theories of Stern (29) and Freud (6) will be obvious.

II

In developing a theoretical model which deals with perceivers rather than perception we begin with a central postulate: The person is a self-regulative system; though dynamic, this system is *quasi-stable* and *continuous*. It must continually bring into harmony needs, impulses, and wishes, and buffer these turbulences from within against limitations from without.

We assume, then, that a person is always in a state of motivation, that is, never without purpose. But—while much of his behavior *is* concerned with achieving objects or states which are desirable or satisfying, underlying this there is a larger motive of maintaining equilibrium between inner strivings and the demands of reality. This prime motive is closely related to the psychoanalytic concept of defense and is the basis in that theory for the defense mechanisms a person develops which give him a recognizable and consistent behavioral face. Out of this we can now derive a corollary to our main postulate. Defenses develop pervasive, stable, and autonomous status: From the interlacing of wish and reality-press a system of "normalizing" mechanisms emerges, typical of the person, which becomes independent of its origin in development. These mechanisms set the mold of experience. We may therefore study them not only in particular conflict- or need-related situations but in any circumstance of adaptive response and in the functioning of any response-system. The perceptual system, which provides for detection, selection, and control over stimulation, is instrumental for the never-ending task of equilibration. Its diversity should therefore reflect personal defensive solutions.

In concentrating on the defensive aspects of perceiving we overlook for the time being its role in direct wish-gratification, nor do we give proper regard to its creative and synthetic aspects. A complete theory could not ignore these. But our immediate task is to look for patterns in perception which reflect typical adaptive solutions, and for the present this aim alone guides the construction of our model of the person as perceiver.

At the basis of our model we place, not *stimulus* attributes, as in conventional schemes, but *functional* ones. These are qualities of the perceptual apparatus through which it selects and organizes stimuli. We choose to call them *"adaptive properties,"* for they are the means we have for fending off, choosing, and admitting stimulation from the welter of the outside world which, with free entrance, would traumatize and overwhelm us. These properties do not comprise the act of perception itself but serve as convenient "categories" by which to view it. Any perceptual act may be described in terms of one or many of these properties, and it becomes a problem of experimental design to devise situations which emphasize one or another, or eliminate one or another. As examples, we may mention briefly: The requiring of a certain minimum level of stimulation to be sensed *(absolute thresholds)* and of a certain increment in stimulation in order for a difference to be detected *(differential thresholds);* the experience of phenomenal *constancy;* the speed of perceptual organization *(organizing time);* and, among others, the ordering of sense-impressions in accordance with a reference-level, a process which we have elsewhere termed *"schematizing"* (12) and which we shall describe briefly later on. These channels of selection are common to all people and may be studied in any sense-modality. However, they are not fixed for all people. Great individual differences in some of these properties have long been known; and it is our claim, supported by studies of individual differences in the schematizing property and in threshold functioning, that these reflect, not chance variation or experimental error, but express controlling forces of integration such as defenses. And, of course, since they serve the person, their functioning should vary among people as preferences for adaptive solutions vary.

Some of you may question whether such "peripheral" events as absolute and differential thresholds serve central dynamisms. There are findings which seem to support this notion of central participation. In the foreground are Slater's careful study (27) showing a deficiency of visual acuity in neurotics and Eysenck's important finding (5) that dark adaptation is impaired to differing degrees in hysterics, anxiety states, and depressives. Malamud and Nygard's discovery of differences in the cutaneous sensitivity of psychoneurotics is also important for us (19). Our own findings on sensory changes in the hypnotic state support the belief in central regulation. We reasoned that if central factors *do* operate, then an altered ego state might disclose altered thresholds. Clear-cut changes did in fact appear under hypnosis though, as we must restate, the pattern of this change varies among people, suggesting that prediction of single patterns may prove to be feasible *only* when the distinctive personality is understood.

The neurophysiologist also reassures us. The discovery by Dusser de Barenne, Garol, and McCulloch (**4**) and by Gellhorn and his associates (**1, 7**) of cortical areas with suppressor functions directly reinforces the possibility of central control of receptor action. Barker and Gellhorn conclude: "The significance of the suppressor areas can no longer be confined to the regulation of cortical-spinal motor activity, since they also have a profound influence on the reactivity of the cortex to afferent impulses" (**1**). In effect they argue that stimulation of such suppressor areas can diminish or suppress the effect of receptor action. This exciting finding provokes a host of questions. One of the foremost would be: What are the activators of the suppressor areas? Are autonomic stimuli important here? If so, we would have a link with psychological findings concerning emotional and temperamental correlates. We have Kennard's statement that functional interrelations between the somatic and autonomic nervous systems "can be demonstrated at many levels of the nervous system from peripheral nerves to cortex," which also gives wider scope to the study of so-called "peripheral" sensory phenomena (**11**). Eysenck in fact favors autonomic disturbance as an explanation of his results.

We must break off this digression now and return to our main current. We should pause here to emphasize that we do not try to explain with an adaptive definition the processes to which our properties refer; that is to say, the mechanism of a short organizing time or a high threshold is not explainable by its adaptive significance. The result of a process does not explain its cause; this must be sought in the process itself. In looking to neurophysiological concepts of suppressor effects to account for higher level control of sensory threshold response, we pay respect to this principle. To admit this rule, though, does not lessen the importance of queries concerning purposes and ends. These suggest new relationships, particularly in the area of motivational variables and defenses, the type of over-all directives which we have been emphasizing. Such relationships as these could appear only through questioning the "what for" of perceptual responses.

Our schematizing studies are a case in point. In one of these (16) people judge lifted weights by the method of single stimuli. We find that they all become "attuned" to the series; they develop a reference level by which to size-up each weight. But when the series of weights is, without warning, made much heavier—say from a 100 gram average to a 300 gram average—people vary widely in how quickly and how flexibly they can give up the previously fixed level and rebuild a more appropriate and accurate one. Thinking through some possible adaptive consequences of these differences, we were led to the finding that inflexibility of reference-levels often appears in a setting of paranoid tendencies in rigid defensive structures. Here, then, to a psychophysical process described long ago in its general aspects by Wever and Zener (32) and others, we are able to give new implications by considering it from the standpoint of individual differences.

Our conception of the adaptive property permits us, now, to look with new eyes upon diverse data, past and current, in perceptual psychology and to digest much of them into a single framework. It might also help us to close some gaps in current findings on the relations of perception and personality.

In general, the keynote of classical work in psychophysics has been not so much the apperceptive content of experience but careful analyses of the formal process. In the past such neutral laboratory data on formal processes have been flagrantly ignored by the personality theorist. They seemed somehow to be true of the biological organism but certainly not so of the *"person."* Yet in clinical practice, we assume as a matter of course that formal processes of a response as well as content, say to a Rorschach card, refer to styles of adjustment. Why not, then, of a psychophysical response? The psychophysicist may have valuable information to offer us about basic ego processes. In a previous paper we described how some classical findings, in addition to those we have already mentioned, may be reformulated as hypotheses relevant to personality theory (12). For example, the constancy of the DL (Weber fraction), as Stern first pointed out, has adaptive significance, and the narrowness or breadth of the range of constancy may also correlate with over-all directives of personality functioning. Accepting this view of formal properties, our perspective broadens, and in the future it might not be impossible to relate in the same theoretical framework a person's Weber fractions and the data from his analyst's couch.

Our interest in formal processes also points up anomalies in current research. In Bruner's as in Murphy's provocative experiments *individual differences* in "autistic" effects puzzle us. We wonder why just so much "accentuation" should occur and no *more* for a given subject, and why this amount differs from one to another; what factors limit the range of autistic effects? Is the limiting factor solely the intensity of the need? Not likely. As we have said before, the *way* that formal processes are employed by the person may hold the answers to some of these questions. In this connection, trace concepts and in particular such constructs as "trace assimilation" postulated by Lauenstein (18), "sedimentation" by Köhler in his consideration of time-error (17), and the Gestalt formulations of isomorphic processes, should not be overlooked by a functional theory. This involves considering the consequences of variations in these processes for adaptation and ego economy. One possibility here is that isomorphic processes are themselves the mediating structure which qualifies the effects of needs and drives. Individual differences in basic formal properties and their isomorphic correlates may hold the key to some of the riddles raised by current studies of values and needs.

From this basis in adaptively defined properties we can now examine meaningfully the range and variety of responses—individual differences—within any single property. We have found large quantitative and qualitative variations in the several perceptual properties we have studied. But fortunately we can classify these into a limited number of categories or, as we prefer to call them, *modes* of response. Our modes, then, are "typical" response patterns within a single perceptual property. Some examples will make this clearer: We found that in our size-estimation experiment some people typically overestimate while others consistently underestimate; still others are quite accurate. We would consider these three *modes* of the particular property which this experiment isolates.

A more recent study permitted us to look at various sides of schematizing behavior and offers good illustrations of response-modes (13). Our experimental situation used projected squares which constantly but gradually changed in size. There were fourteen squares in all. The smallest five were seen for a while and judged for size, and then, without the subject's knowing it, the smallest square was taken out of the series and a next larger square was added to the upper end. This subtraction and addition continued until all fourteen squares were exposed. We then asked about each subject's responses: How accurate and flexible was his shifting behavior—did he notice the changing stimuli, and was he able to record the change accurately? What was the direction of inaccuracy? How variable were his judgments? How refined was his scale? In all of these parameters striking tendencies or what we call *modes* of response appear. We have time to note only those which appeared in "accuracy and flexibility of shifting." Three main modes are present, though others appear in lesser frequency: (1) progressive underestimations of size, a phenomenon which we call "adaptive lag"; (2) a period of lag with sudden recovery to a more appropriate reference level; (3) consistent and flexible shift in reference level, corresponding well with the actual shifts in the sizes of the squares.

These variations suffice to illustrate our concept of modes. Modes are variants in the way perceptual properties are actually put to work by particular persons. The adaptive meaningfulness of modes is not always self-evident, of course. While it is tempting to speculate about the personal usefulness of a high threshold or slow organizing time or the consequences of "adaptive lag," not all modes are so easy to deal with. But as we climb our hierarchy to successively higher levels, this may become easier.

We have offered a way of understanding how people see, hear, and feel in terms of speculative adaptive definitions of the very foundation stones of perception—its formal properties. Perhaps it seems that we are peering too microscopically at tiny and isolated details of response. But we do not stop with the delineation of modes of response. Our chief interest is not just modes of response but the way these modes, within and among perceptual properties, pattern themselves. This is a level of analysis which we have not yet discussed, that of empirically found clusters, patterns of modes, to which we give the name *syndrome*.

It will help to describe the earmarks of these patterns which we call syndromes. They are groupings of *modes* of adaptive properties, which have a common quality: They contain within them a single perceptual attitude (an *Anschauung*) or integrative principle. They cut across adaptive properties. Notice that we do not try to reason from a particular score or response to an integrative principle of either perception or personality. Rather we seek these principles in how various modes order themselves.

We will illustrate the derivation of a syndrome with two adaptive properties and how they interrelate, though a syndrome can include many properties. The concept of syndrome grew naturally out of our search for ramifying ties of schematizing modes to other perceptual behaviors. We have described two extremes of shifting behavior, one in which considerable lag occurs and the other in which the shift is appropriate and accurate. This suggested to us that at the one extreme of "lag," judgment is guided almost entirely by "preconception" (or "background"), e.g., that "the stimuli are all small," in contrast to the other group who realistically evaluate the special qualities of *each* stimulus in relation to the rest. A clinical translation of this type of "lag" phenomenon would be the hysteric's tendency to call quite different things the same based on superficial resemblances (as, for instance, that all Rorschach cards are the "same" because they are all ink blots). We then thought that our "lag" subjects might be more susceptible than others to the contrast illusion, reasoning that the "pull" which a background has upon an enclosed gray stimulus might be much greater for them. This hypothesis was borne out, though we find that other facets of schematizing, as well as lag, add precision to how well we predict the contrast effect from schematizing behavior. A relationship thus appears between two sets of behaviors—schematizing and contrast—in terms of which we can group people, and eventually we may be able to define from this relationship one distinct perceptual attitude of the kind we are groping for. In this study we are also working on hunches that particular patterns of schematizing will predict functioning in other adaptive properties such as organizing time and constancy. In this way by widening our hypotheses to include new adaptive properties we hope to demarcate syndromes.

The syndrome, then, is an important boulder in our theoretical structure. The perceptual attitude it conveys we would like to regard as an axis of perceptual organization. Please observe that we think of these syndromes as *preferred* styles of expression rather than *required* ones. Ours is not a fixed scheme of what the person *must be* perceptually but what he *usually* is. Here we can show another link to work done in other traditions in the fields of perception and personality. We believe that at the syndrome level in our theory it is possible to reintroduce explanatory principles as diverse as the integrate-disintegrate attitudes postulated by Jaensch (10), the physiognomic attitude by Werner (31), the repressive attitude by Freud (6), and those recently defined by Thurstone in his factorial study of perception, such as "speed and strength of closure" (30). The description of an *Anschauung* in spatial orientation appears also to be the intent of Witkin's important studies (33). Such concepts as "repressive," "integrative," "physiognomic," and the like may describe distinct "perceptual attitudes" of the sort which we presuppose to lie back of our syndromes.

This opens another avenue of study. Not only can we search upward for the syndrome from adaptive properties below, but we can also take what frequently is an easier and more interesting route: reverse the entire procedure and start with "perceptual attitudes" which on the basis of the work of others seem to refer to syndromes as we have defined them. Take, for example, the integrate-disintegrate polarities of Jaensch: if we divest these of their catchall extensions, we can treat them as a hypothesized attitude for which a syndrome could be sought. Defined perceptually, the hypothesis refers, on the one pole, to people whose experience is marked by a high degree of *interpenetration* of stimulus influences, and on the other pole, to those with the ability to maintain a high degree of stimulus *differentiation*. We could use to advantage the many perceptual techniques developed by Jaensch which involve, as do many of ours, constancy phenomena, organizing time, intersensory effects, and so forth. Other "borrowed" attitudes can be treated in the same way, by finding the adaptive properties and modes they comprise and by studying their ties to each other.

Thus, perceptual syndromes reflect for us the integrative principles which give character and distinctiveness to perceptual life. If, as we assumed earlier, the perceptual system "represents" the person, we should be able to use these principles to describe personality. What we mean by the structure of personality, as we said before, might actually be the constellation of such principles as these. Our own work has not yet reached the stage of allowing us to deal with syndrome-*clusters*, but those studies we have made with single properties encourage belief in the principle of "representation," for even in the single property of schematizing, as we have seen in our study of paranoid persons, prediction is possible to other levels of behavior. For the syndrome described above (involving at present schematizing and contrast behavior) we are also comparing the clinical records of extreme cases. The present stage of this analysis also gives more body to the belief that these perceptual extremes reflect more general behavior tendencies—people who lag and are prone to a high contrast effect show typical differences in their life adjustment from those who shift appropriately and show moderate contrast. We would expect, therefore, that the prediction of general behavior patterns from perceptual behaviors will be even more exact when we are in the position in the future to describe people in terms of several—a cluster—of perceptual syndromes. If we are right, then, in expecting that syndromes in relationship to each other are likely to touch a side of the personality's radix, the salient question for us to think about next is how to depict such syndrome-clusters.

One lead we can follow in solving this problem is that people tend to group themselves along qualitative axes or syndromes. Here the typological method might be a useful instrument to represent constellations, and we aim in fact to build eventually an empirical typology on the basis of these natural clusters. But let us be clear on several points: (a) the types we expect to draw are groups of individuals with similar syndrome-clusters who we can say, therefore, have similarly organized perceptual systems; (b) they are inductively rather than deductively derived; (c) they do not violate individual uniqueness but bring together similarly organized people in a way which permits commonalities to highlight rather than obscure differences. These types are not intended to "explain," but rather to suggest hypotheses and so quicken the study of individual organization.

There is another reason to proceed typologically. We are, of course, very much interested in how generalizations made at the clinical level relate to those we draw from perceptual data. Now, the thinking of clinical science today is largely typological in nature though often crude and not always self-consistent. But if we would use and understand the clinician's data to best advantage, we must be prepared to think in methodological terms similar to his. We may seem to imply it, but we really do not intend to jump the gap between clinical and experimental methods with correlation coefficients. Rather, we want to use the clinical material in a broader way: to look at the clinical picture of each person and fathom its meanings in terms of his perceptual type. Perhaps clinical material can also help explain differences *within* perceptual types. On the other hand, diagnostic clinical types (e.g., obsessive-compulsive, hysteric, schizoid, etc.) might themselves be directly translated into perceptual hypotheses. For example, hysterical defensive structures, clinically defined, might actually correspond with a quite unique pattern on the perceptual level which contrasts, say, with that to be found for obsessive-compulsives.

Available statistical methods are not altogether suited to the tasks we have outlined, especially that of "typing." A major exception is the technique of inverted factor analysis or "Q" technique described by Stephenson (28), in which persons rather than tests are the correlated variables. This innovation promises to be a real help in constructing empirical typologies. Conventional factor techniques and "syndrome analysis" as used by Horn (9) and others of Henry Murray's associates (25) are perhaps usable to extract syndromes. With all of these, however, we cannot yet tell whether they will fit the severe requirements of a *verstehen* approach which regards scores as multi-determined and therefore inequivalent from person to person.

We have tried to outline a conception of the perceiver which respects his unity and continuity. In this we have chosen to be programmatic and extensive rather than intensive and definitive. The framework suffers from serious omissions in its understructure. For one thing, we have stressed only the enduring or abiding character of perceptual organizations, assuming with Murphy "a stable coherent system (which) though it is pushed here or there . . . comes to rest in its familiar form" (22). Our own experiments now follow this line of thought. But the scheme ignores the qualitative changes in perception which may come about under special stresses and special motivations. In limiting ourselves for the present to the over-all motive of defense we have also ignored the push and pull of resources, wellsprings of energy, which are variously called "growth force," "ego strength," and "creative energies."

These are temporary omissions rather than crucial oversights and need not essentially detract from the workability of our scheme. We hope it offers a beginning basis for a person-oriented theory which will have predictive power for the individual. In our search, we have been forced to lead you on to ground which we ourselves have not fully explored, often without expressing the cautions we feel. But we have tried to separate those facets which are merely promising from those which have proved their usefulness. We have described an arc in which psychophysics and personality theory touch. From our original query of "Where is the perceiver in perceptual theory?" our course took us back and around through small details of structure, to the capturing and categorizing of modes and syndromes, up finally to perceptual *Anschauungen,* where we *hope* to see a true image of the person.

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DISCUSSION

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My one plea in connection with these papers would be for a more explicit statement of the neurological brain models which are implied. I make such a statement with surprise because for many years I have objected to what I have conceived to be premature neurologizing. It seemed so obvious that psychology was handicapped and led astray by the narrow neurological concepts which it took over uncritically from, say, the physiologist's account of decorticate frogs. And so, for a long time, I have feared any attempt to bolster psychology by trying to base it on the as yet observed or postulated facts of neurology. And I still have qualms about present-day neurology, even if it be called "Cybernetics."

Nevertheless, under the influence of the Gestalt movement, I have come to realize that a tentative, hypothetical brain model is in fact inevitable and to be desired. Further, I would now coin the term "pseudo-brain models" to cover such hypotheses, not to cast aspersions on them or to suggest that such models should not be as solid and as consonant with known neurology as possible, but rather to emphasize that if they are to be comprehensive enough for *our* purposes they must at the present time be very speculative, very tentative, and often only pseudo neurological in character. I would cover, that is, under the term "pseudo-brain models" not only the relatively strict, physical, electrochemical, and tissue-grounded hypotheses of Wertheimer or of Köhler and Wallach but also the far less tissue-grounded "life-space" models of Lewin or of myself. But the important point, as I see it, is that a model (whether almost good physiology or almost wholly "pseudo") has certain specific intrinsic properties attributed to it by its authors. And it is by following out the consequences of these attributed properties that one is led to wider predictions than those already found empirical relations with which we start. A model provides a conceptual substrate —a substrate which is endowed by its author with certain intrinsic properties of its own. And, if the model be a happy one, then we are led by it to expect new behavioral relations which we would probably otherwise never have thought of.

Or to put the whole matter another way, I am now convinced that "intervening variables" to which we attempt to give merely operational meaning by tying them through empirically grounded functions either to the stimulus variables, on the one hand, or to the response variables, on the other, really can give us no help unless we can also imbed them in a model from whose attributed properties we can deduce new relationships to be looked for. That is, to use Meehl and MacCorquodale's distinction, I would now abandon what they call pure "intervening variables" for what they call "hypothetical constructs," and insist that hypothetical constructs be parts of a more general hypothesized model or substrate.

Let me indicate how this criticism applies to each of the three papers.

Wallach speaks of "memory traces," "associative connections between memory traces," "present primary perceptual processes," "interaction between the present primary perceptual process and memory traces," and the influence of "central factors" as being "mediated" through the memory traces.

"Memory trace," "present primary perceptual process," "central factors," and the assumed "mediations" are, as we all know from the other writings of Wallach and of Wallach and Köhler, conceived as co-ordinated to very specific neurological processes. And it is by postulating the intrinsic laws of this assumed underlying neurology that Köhler and Wallach have explained their already discovered perceptual phenomena and have been led to predict new ones to be looked for.

In the present paper Wallach seems to have been expanding this assumed underlying neurology to take care of central factors, such as needs, values, and sets, and to suggest how such central factors interact upon perceptual processes. In so doing, he has implied an extension of the previous Köhler-Wallach model. But I would like to know more specifically what this extension is.

Bruner and Postman have presented a stunning review of all the recent experiments on the effects of motivation and set factors on perception and have suggested ways in which such factors can be operationally treated as parameters in the equations to be found between formal stimulus factors and resultant perceptions. More specifically, they have suggested certain mechanisms of "accentuation," "level of adaptation," "availability of trace systems," and a "hierarchy of response thresholds." What I am asking is that they follow their own prescription that "it is almost impossible to indulge in too much speculation," and that they develop and state their own pseudo-brain model (which they have been implying but not stating), and show us just how, in terms of this model, their constructs of accentuation, adaptation level, traces, availability of trace systems, and hierarchy of response thresholds can be expected to interact and produce the final perceptual behaviors which they do. Would or would not their model turn out to be different from that of Köhler and Wallach, and would it or would it not lead to different sorts of further relations to be looked for?

Finally, Klein's most important paper in which perceptual "modes," "syndromes," and *Anschauungen* are to be correlated to, or actually substituted for, personality types still leaves one of my temper to suspect that rapid progress in the further discovery of such correlations or identities will be facilitated only when and if he, or some fellow worker, succeeds in suggesting some pseudo-brain model, or substrate, in which the constructs of perceptual modes, syndromes, and *Anschauungen* are imbedded and which substrate therefore leads to expectations as to further necessary perceptual-personality relationships.

Finally, in conclusion, this demand for models may in each case seem to be asking too much. Any model, we can suggest now, must inevitably be inadequate and but sketchily related to the meager sets of empirical evidence now at hand. Nevertheless, my final plea is that conscious, even though bad, theory is better than unconscious theory. We all do have models in the back of our heads when we collect systematic sets of data. Therefore, we should stick our necks out so that both we and others can see what these models are. For I have faith that the more explicit we can become about our theories, the more fruitful they will be, and also the more likely it is that we will be ready to drop or change them when they no longer prove useful. Or, to put it another way, the sin of "rigidity" is, I believe, more apt to function in the unconscious than in the conscious.

DISCUSSION

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I am sure I speak for the audience in saying that our first response is one of deep gratitude to our three participants for the extraordinary vitality and creativeness of their thinking. It seems to me that while emphasizing different aspects of their fundamental problem, they have yielded material offering a most stimulating integration.

To begin with some questions of critique, I would first stress what I think is the rather unfortunate terminology which Bruner and Postman have suggested. They have made a vital and important distinction between groups which they call *functionalists* and *formalists.* I doubt, however, whether form and function permit this sort of contrast. Everywhere in nature and in human life function leads to form, and form becomes an epitome of function. The exquisite pattern of a seal's body and movements, the amazing majesty and power of the rhythms of the tiger's tread, are exemplifications of form which have developed through time as exquisitely serving the needs of function. In the same way, a Greek vase or the modern stratoliner derived from the crudities of the plane which first flew for the Wright brothers at Kitty Hawk tells the story of progressive molding of form to meet the needs of function. The term formalism, moreover, suggests, if not arsenic and old lace, at least the dry rot of Miss Havisham's existence in *Great Expectations,* the reduction of life to a sterile and arid, and therefore artificial, symmetry. The term formalism has become a "bad word" in many quarters where critics emphasize vitality.

It would be sounder, I believe, to treat the people whom Bruner and Postman have in mind as occupying different points on a continuum, where, without invidious distinctions, they emphasize varying *degrees of complexity.* The study of a simple limen by the methods of psychophysics may at times be pursued within a very simple frame of reference, and at times within the more complex one suggested in the present symposium. But degrees of simplicity have their place in science, and the simpler need not always be out of place.

Despite the emphasis upon integration which runs through all these papers, it is my own feeling that a wedge is still being driven artificially into the organism at a number of points and that distinctions have been made for which empirical justification is scant. For instance, if I am correctly following Dr. Wallach, he believes that there is a basic distinction to be made, as indeed his earlier work suggests, between a primary perceptual process and a memory trace. He evidently feels that we are dealing here, not with questions of emphasis, but with basically distinct processes. It seems to me that the current research on the molding of perception by all that the individual is, at the time he perceives, indicates that the primary perceptual process is in itself an interaction—that the water drop is shaped, so to speak, as it strikes the surface of the pool, and that the surface of the pool is in some measure shaped as the water drop touches it. If he means only to distinguish between the thing impinging and the thing upon which it impinges, and has a place for the fullest reciprocity between them, I have no grounds for dissent. But the whole discussion of figure and ground and of related problems suggested to me that he believes a primary perceptual process can occur before there is "contact" with "memory traces," and of this I can see no evidence in the perpetual interflow of processes which occur in perceptual response. If I have mistaken his meaning, I am sure he will generously forgive me and redefine the problem.

Likewise in the paper by Klein, I felt there were at times suggestions indicating the isolation of one response from another. In speaking, for example, of attitude as a controlling factor in perception, he seems to think of attitude as a process independent of the biological individuality of the perceiver. Why could not the common factor which holds these "types" together prove to be a somatic predisposition molded through social living into a particular contour or formal process, thus reflecting both biological and social factors simultaneously and in their fullest interpenetration? I felt through all the papers, in fact, a certain naïveté with regard to the nature-nurture issue, a neglect of such biological problems as those which Kretschmer and Sheldon have broached, and a neglect of broader social parameters of the sort discussed in the sociology of knowledge and in the school led by Muzafer Sherif.

One thing that I liked very much in the Klein formulation was the conception of hierarchies, in which we may regard the elements at the bottom of the pyramid as relatively distinct and capable of isolation, but in which, as we ascend, the pyramid involves tighter and more complex modes of interrelation. This way of thinking enabled him to raise the question whether events occurring near the top of the pyramid can actually act down, so to speak, to modify the simpler phenomena near the base. His mobilization of evidence along this line is, I think, of the profoundest importance for psychology. It is important not only for personality and perception but for everything else. Why could we not, through the study of action currents in receptors, in sensory neurones, and by other means, begin to test empirically this action from higher to lower?

This way of thinking in terms of a hierarchy also permits us to place all of our types of research somewhere in relation to the pyramid without invidious distinctions between them. Beginning at the bottom, we would have studies of the functions of receptors and sensory neurones. A little higher up, perception would appear in relation to the central nervous system, as brilliantly exemplified in such recent work as that of Köhler on the direct currents in the brain involved in figural aftereffects. At a higher level we should have perceptual responses dependent upon the whole organism as an organism. Here we should have the interactions of central and autonomic nervous systems in the simpler autistic responses and evidence as well of numerous biochemical factors; but we would still be looking upon the perceptual response as simply a function of the individual. Still higher up in the pyramid there would be a place for consideration of the determination of perception through fuller specification of modes of interaction between organisms and their environments, including of course interactions of organisms as members of social groups.

It could, of course, be said that these higher levels of integration still leave us with the fact that it is the individual organism as such that does the perceiving; but what we are concerned with here is the hierarchy of variables which have to be controlled in getting a real grip on the perceptual response and in predicting what that response will be. For a long time to come, I think, research will certainly have to be field-oriented. We will have to consider organism-environment interaction in all its full complexity. Perhaps, indeed, at the apex of our pyramid there will have to be a place for a kind of perceptual dynamics in which the biological and social realities, however complex they may be, are brought to a single point, in the attempt to see why the particular thing that you or I perceive at the particular time expresses not only everything that we are but everything that is acting upon us.

The purpose of such a hierarchical scheme, however, is not primarily aesthetic. Its possible value depends on the question whether it helps us to predict more accurately what the individual perceptual response will be. Each of these approaches is good when it works. The trouble with the approaches lower down on the side of the pyramid is simply that in many situations their predictive power is poor. I could have wished, therefore, that all of our speakers had told us how we are to test their new conceptions *by prediction* in such a way as to show what you can do with these new methods that you could not do with the old. I wish, for example, that Klein had told us, after his exquisite analysis, just how you verify the adequacy of his approach in the laboratory by predicting perceptual responses which could not have been predicted by any other means. Stimulating and vital as all these approaches are, they are to be taken not as new dogmas but as ways of working, to be validated by their results. Actually, I believe that validation can and will be secured. But I hope that in the present feverish interest in personalistic studies, in projective tests, and in research upon ways in which personality influences every specific part function of the organism, we shall develop the critical apparatus, the concepts and methods necessary to steady our steps and enable us to be sure whether we are on the right track.

Indeed, it is because of the rich emphasis upon experimental methods and the disciplined consideration of the need for research that I think this symposium as a whole has blazed so magnificent a trail. The speakers have put a tremendous lot of good solid stuff into their brief formulations.

What they have really done is to deal simply and cogently with some of the most elusive of all psychological problems, problems which in William James's day were so "tender minded" that they eluded the experimental attack of the "tough minded" scientists of his period. In Egon Brunswik's remarkable comment on the change of temper in psychology we have this phrase:

"In conceiving of the interest in higher mental functions in general as the product of a 'tender' frame of mind, one may perhaps say that act-psychology has dealt with the tender in a tender way, whereas we methodological positivists try to deal with the tender in a 'tough' way. In a nutshell, this combination is perhaps the crux, and the great challenge, of modern psychology in general."

This symposium seems to me an excellent example of successful dealing with the tender in a tough way; an admirable redefinition of the most subtle and difficult problems in such a way that an appeal to unequivocal experience in life and in the laboratory may serve to make clear what the issues really are.

DISCUSSION: REMARKS ON FUNCTIONALISM IN PERCEPTION

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The type of functionalist thinking espoused by Bruner and Postman (4) may serve as an occasion for a more general discussion of perceptual functionalism and its relation to formalism.

VERIDICAL VS. SUBJECTIVISTIC ASPECTS OF PERCEPTION

One may readily agree with Bruner and Postman's identification of formalism and Gestalt psychology. Perhaps some of the Gestaltists will not like this, but I cannot help but see the long-range core of Gestalt psychology to be the law of prägnanz, of which in turn the principle of closure is but a special case. While closure is without doubt all important in creative problem solving, it does by the same token interfere with what Bruner and Postman have aptly designated as the "veridical" aspects of perception. Retouching of form may beautify the world, and it may be helpful in other ways; at the same time it is inaccurate, and it cannot in and by itself reconstruct environmental realities. Tendency toward geometric regularity and good form does not in essence go beyond the tradition of the study of subjectivistic illusion as inaugurated in the Graz school of the 1890's. The dynamic interaction in a closed brain field which is assumed to underlie the tendency toward prägnanz amounts to a kind of self-sufficient encapsulation of the perceptual system. Its inherent formalism[1] constitutes a scientific analogy to philosophical rationalism and apriorism. The Kantian heritage in Gestalt psychology has been acknowledged especially by Metzger, one of the leading orthodox second-generation Gestaltists (**19**, pp. ix, xvi).

The established concept of functionalism, on the other hand, includes some aspects of perception other than Gestalt which Bruner and Postman seem to be inclined to subsume under formalism. I shall first discuss the veridical functions of perception. They may be stratified into three major subdivisions, in accordance with the depth level of cognitive attainment.[2] Most narrowly confined of the three is classical psychophysics, including peripheral, sensory-neural technology. Since this type of approach concentrates on relations with the "proximal" stimulus impact, it does not reach the biologically most relevant layer of manipulable physical and social objects. It is thus comparatively insignificant from a functionalistic point of view, except indirectly.[3]

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[1] The onesided formalism of Gestalt psychology proper—that is, of the so called Berlin school centering about Wertheimer, Köhler, and Koffka—has also been noted by the leading experimentalist of the rival Leipzig group, Sander, in his far too little known summary of Gestalt research before the German Psychological Association in 1927. According to Sander, the "geometric-ornamental" tendencies on which the Berlin group concentrates represent but a relatively primitive phase of development, especially also in the "actualgenetic" emergence of single perceptual responses (**20**, pp. 65 ff.).

Probabilistic Functionalism in the Study of Thing Constancy

Next are the perceptual thing-constancies. On the European continent, this topic was as much a major fashion of the between-the-wars period as, say, conditioning in this country. In the field of color, it was set up in a style all of its own by David Katz, Bühler, Gelb, and others. Thing-constancy research concentrates on measurable correlations between distal stimulus variables and organismic responses. These correlations are eminently relevant to orientation in the environment and thus to adjustment in the most specifically functionalistic, that is, utilitarian sense of the term.

Contrary to a widespread notion, thing-constancy is neither historically nor conceptually a part of Gestalt psychology. Emphasis on "framework" and other formalistic schemes of balance and equilibration, typical of the Gestalt psychological handling of the constancy problem, can be shown to be inadequate in dealing with the distal relationships involved; this also includes the adaptation-level theory of Helson (referred to by Bruner and Postman). Even a thinker as personally close and as sympathetic to the Gestalt point of view as Fritz Heider has stressed the fact that Gestalt psychology is not properly concerned with "distal" object relationships (**13**).

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[2] Concerning the concept of perceptual attainment, see **5**. For a more recent exposition of the various peripheral-proximal and distal strata and the mechanisms involved in the attainment of the latter, for a schematic outline of the corresponding historical sequence of structuralism (classical psychophysics), Gestalt psychology, and objective (probabilistic) functionalism as discussed in this paper, and for the reverberations of this shift upon experimental design, see **8**, especially the summary presented in Fig. 7 (pp. 28 ff.), and Fig. 10; see also **10**.

[3] This should not detract from the fact that, as is being pointed out in this symposium by Klein and Schlesinger (**16**), threshold tests and related devices of classical psychophysics may have important diagnostic value in the framework of a differential—rather than more strictly functional—psychology (for this latter distinction, see **8**).

A resolution of the inherent difficulties can be effected only by proper recognition of the role of "cues" in perception. Although this is essentially a semantic concept, it has a symmetrical counterpart on the overt behavior side in the concept of "means" as stressed especially in Tolman's purposive behaviorism. In emphasizing the important role played by assemblies of vicarious, mutually interchangeable cues—in independent parallelism to Hull's simultaneously developed emphasis on "habit-family-hierarchies" in overt behavior (**15**)—the present writer (**5**)[4] seems to have placed himself not only beyond, but in opposition to established Gestalt doctrine. In reviewing my book, Metzger declared the revival of the emphasis on cues a return to an outmoded atomism, intellectualism, and associationism. Among those more sympathetic, Boring (**2**) has aptly characterized this revival as a "modern equivalent" of Helmholtz's doctrine of unconscious inference, apparently hinting at the existence of important differences along with the obvious similarities.

In trying to nail down the changes effected between the corresponding stages of what must be considered a spiral-like recurrence of analogous principles on different levels of methodological scrutiny,[5] I should like to refer to a recent paper (**9**) in which I tried to demonstrate a discrepancy between perception and thinking proper in strictly functional terms. The traditional method of introspection combined with plausibility arguments was replaced by a differential analysis of the statistical distribution of errors. A task in size constancy was presented, in as nearly identical versions as possible, once to direct perceptual observation, once as an arithmetical reasoning problem. Perception proper was in our example characterized by a compact and fairly normal distribution of accuracy, with relatively few perfect answers but also few drastic errors. The explicitly intellectual approach, on the other hand, yielded in our example a considerable proportion of "on-the-dot" correct responses; these were accompanied, however, by isolated bars of very crude errors representing typical confusions with potential alternate tasks, as well as by erratically scattered, sometimes way-off and bizarre, mistakes. The latter facts point toward a dangerous one-track quality which may well turn out to be inherent in much of explicit reasoning. In our example, at least, thinking was found to use one or a few relatively foolproof cues in a crudely machine-like system of switches. The more implicit "logic" of perceptual quasi-inference, on the other hand, must here, as in many other instances, be seen as characterized by compromises between a large number of rather insufficient yet stereotyped cues. This injects an element of comparative "stupidity" into perceptual intuition which, from a biological point of view, is made up only in part by the quickness of the responses established in this manner.

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[4] For the relation to Hull just mentioned see (**8**), p. 48; for an illustration of the vicarious functioning of distance criteria in perception as demonstrated by Holaday, see p. 23.

[5] For further elaboration, see **10**.

The error statistics on which this demonstration of functional difference, and thus of the separate identity and the comparative autonomy of perception and explicit reasoning, is based is part of a more general policy of objective achievement analysis which has been developed in the study of the thing-constancies. It is by pointing to this behavioristic feature of modern perception research that we can best become aware of the long way we have come from the introspective act-psychology of Brentano and of other early European and American functionalists. In act-psychology the "object" is not defined by measurable variables in the environment but possesses a rather dubious ontological status in a no man's land essentially derived from introspection. It thus has no adjustment relevance and does not directly contribute to functionalism as we understand it today.[6] The same holds for the "phenomenological" interest which stands at the beginning of the study of color constancy. It must be stressed, however, that act-psychology and phenomenology are important, and perhaps indispensable, preliminaries to molar research of the objective kind.

Again, there is an analogy between my own suggestion to replace

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[6] It is for these all-important differences between the "subjectivistic" and the "objectivistic" approach that the "probabilistic functionalism" suggested by the present writer (see below) is not to be put in one class with act-psychology, as a prominent author seems to have recently implied (**14**, p. 261). Aside from similarity—in form, though not in content—to Tolman's, Lashley's, and Hull's brands of behaviorism (the last named in its more distinctly molar pre-axiomatic phase) as in part pointed out below, there is an affinity to Woodworth's more purely programmatic dynamic psychology, whereas the relationship to the introspectionistic phases of functionalism is no more than historical. See further **10**.

Brentano's and James's introspectionistic "intentionality" by "intentional attainment" as operationally defined through experimental data in thing-constancy in combination with the mathematical theory of relations, and the redefinitions of such terms as "purpose" or "hypothesis," by Tolman and by Krech, in the framework of operational behaviorism.[7] In conceiving of the interest in higher mental functions in general as the product of a "tender" frame of mind, one may perhaps say that act-psychology has dealt with the tender in a tender way, whereas we methodological positivists try to deal with the tender in a "tough" way. In a nutshell, this combination is perhaps the crux, and at the same time the great challenge, of modern psychology in general.

Since the establishment of veridical distal environment relations is contingent upon the trustworthiness, or statistical validity, of cue-to-object relationships, and since this "ecological" validity is in turn essentially limited by the erratic nature of the environment, attainment of distal variables can never be better than probable. Environment-oriented objective functionalism thus is necessarily "probabilistic functionalism" (**5**, **7**, **8**, pp. 47 ff., 54). The limited validity of perceptual cues must also be considered the basic reason for the principle of perceptual compromise, known since the days of Hering, and more recently quantified by such devices as the constancy ratio (**5**). If single cues lack in trustworthiness, there must be compromise between many of them to assure as high object-attainment as possible.

The fact that new cues can be incorporated into the perceptual system in an almost conditioning-like manner has been demonstrated by Fieandt (**11**), while successful learning of probabilistic cues was demonstrated by myself and by others (**6**, **18**). All such acquisition involves contiguity association. While Köhler, and now again Wallach in his symposium paper (**23**), stressed the importance of memory traces within a Gestalt psychology which is in other respects not always favorable to recognition of past experience, we must note that the further argument in Wallach's paper involves primarily the principle of similarity rather than its historical rival, contiguity. Ehrenfels's famous criterion of transposition, later espoused in

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[7] See **5**, **21**, **17**. For a mutual acknowledgment of the symmetrical relationship of thing constancy research and molar behaviorism, see **22**; see also **10**.

Köhler's theory of physical Gestalten, stresses the generality of form-perception and is thus likewise based on the similarity principle, which in turn is one of the oldest and most concise expressions of internalistic formalism in psychology. Study of the thing-constancies, on the other hand, has led, in the manner pointed out above, to recognition of specific and docile commerce with the environment through sensory cues, much in the tradition of English empiricism and associationism, both preparatory movements to functionalism in the utilitarian sense.

SOCIAL PERCEPTION AND REPRESENTATIVE EXPERIMENTAL DESIGN

Broadest and most inclusive of the veridical perceptions is given by social perception. As understood here, this term does not refer to the influence of social factors on perception at large, but rather to the attainment of what may be called covert-distal traits of personality and intelligence in other people by way of, say, intuitive snap judgments. Whereas classical psychophysics and the overt-distal multidimensional psychophysics of the thing-constancies have gradually emerged in European academic tradition, the study of social perceptual achievement has been pioneered chiefly by personnel experts in the framework of American applied psychology, after the first war, and generally is in the direct tradition of the objectively and practically oriented wing of American functionalism.

The fact that social perceptual attainment is notoriously meager must not detract from its inherent intent. On the contrary, it may help in stressing the probabilistic, rather than strictly "nomothetic," character of studies dealing with complex textures of interwoven factors in the physical or social environment. By the same token, the study of social perception brings home in a most natural way the necessity of representative design of experiments as the methodological counterpart of probabilistic functionalism (8).

PERCEPTION, MOTIVATION, PERSONALITY

Let me now turn to the type of functionalism on which Bruner and Postman have concentrated in their paper. It is not utilitarian in the same cognitive sense of the word as are the two branches of distal veridical functionalism just discussed. In stressing the contribution of the organism, more than that of the stimulus configuration, to perception, it shares in the emphasis on non-veridical illusions so characteristic of Gestalt psychology. Since, however, the sources of illusion are now some motivational forces rather than the comparatively trivial disturbances issuing from the stimulus configuration, the types of distortion involved may well be part of an adjustment mechanism of still broader scope than those studied in the psychology of distally veridical perception. In some related cases, such as in the autokinetic phenomenon and perhaps in extreme cases of autism, the perceptions involved are almost without an external stimulus, and there is resemblance to the Gestalt studies of prägnanz under reduced stimulation.

Error, maladaptive as it is in a narrowly cognitive sense, is seen in its productive aspects in both motivational functionalism and in some applications of Gestalt thinking. But whereas the Gestalt point of view remains broadly cognitive in that it stresses "good" error in the course of coolly inventive creativity—by no means free of motivational strain to be sure—recent motivational functionalists seem to be more inclined to emphasize what may be called the "beneficial" role of distortive mechanisms in over-all clinical personality adjustment.

It may be well to note that the crucial step in studying the role of motivation is not the abstract recognition of its importance but the more purely methodological fact that motivation is made actually to vary in the experimental design rather than just being controlled as a constant. Watson knew that no rat would run a maze unless hungry or otherwise motivated; but it was the actual variation of degree of hunger as in the work of Tolman which opened up the study of motivation in behavior. Quite in analogy, the studies on the perception of number with Austrian stamps and Turkish coins (reported in my *Wahrnehmung und Gegenstandswelt* in 1934[8]), and the improved development of these experiments, with American and Canadian stamps, by Ansbacher (1), fall just short of a full-fledged consideration of motivational factors of perception. They stop with the attempt to establish the influence of "learned" values upon perception, with an eye primarily on familiarity. It is only by Bruner and Goodman (3) that varying strengths of motivation toward these values are introduced. While thus our group has established values, they have established the *need* of these values, as a factor in perception. True, that attitude has been varied and found to be influential in our experiments as it had been for some time in the study of thing-constancy proper; but aside from the fact that mere verbal instructions are weak substitutes for a kind of motivation built as much into the person as is Bruner and Goodman's rich-vs.-poor variable, our instructions have in most cases endeavored to shift the direction of perceptual intention from one cognitive variable to another rather than to modify the strength of intent toward one and the same variable.

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[8] On pp. 140-150 of this book (5) there is a relatively detailed account of the otherwise unpublished work of I. Zuk-Kardos with stamps and of A. Fazil with coins.

It must be stressed, of course, that not all influence of motivation on cognition leads away from "truth." If value or need serve mainly as an emphasizer, say, in establishing figure-ground organization, the case is merely one of selection. In such cases of an influence of the value, say, of coins on their perception as found in our own studies or in those of Bruner and Goodman, however, there is actual distortion. On second thought, one may wonder how well the distinction between selection and distortion would stand up under conceptual scrutiny.

In the attempt to amalgamate motivational problems with those of perception it seems possible to go further than is the case in most studies of this kind. As has been pointed out by Else Frenkel-Brunswik (12) one may integrate the two aspects either in a "perception-centered" manner, that is, by adding a motivational factor, such as a need, fear, or reward as a further variable to the traditional core of perception psychology, or else one may borrow the basic categorial structure from personality dynamics. This latter procedure she calls the "personality-centered" integration of the psychology of motivation with that of perception. In her own case, ambiguity-tolerance is in this manner being derived from emotional ambivalence and the degree of its repression as conceived of in psychoanalysis, whereby her earlier work on mechanisms of self-deception plays the role of an added stimulus. It is easily understood that a development of this kind has little to do with the academic line of tradition leading from Gestalt ambiguity, as inaugurated by Benussi in the framework of the old Graz school, to the recent work on probability learning. In all of this latter development individual differences in rigidity vs. flexibility are consistently ignored, quite in the tradition of what Boring has called the psychology of the "generalized human mind" (even though in some instances the generalized human mind has become a generalized animal mind). Rather than being related in their origin, the two lines of development may join after each of them has established itself independently of the other.

CONCLUSION

This leaves us with a total of six distinct kinds of perception psychology, not counting the early, purely introspective so-called functionalism of the act-psychologists. Bruner and Postman thus have hit it on the dot in their nightmarish prophecy of a multifarious tower of Babel, and they have been obliged sooner than they may have anticipated. Actually, differences may be less drastic than meet the eye. The two oldest schools of thought, veridical classical psychophysics and non-veridical Gestalt psychology, are the least functionalistic of the six, although the creative aspects of Gestalt processes must definitely be vindicated in this respect. The remaining two veridical aspects of perception, thing-constancy and social perception, being distal in character, are genuinely functionalistic. They represent the sober objectivity of the reality principle, so much in vogue in modern dynamic ego-psychology though perhaps in temporary eclipse on its own side of the fence in this era of all manners of convergence and crosspassing. Furthermore, the probabilistic element inherent in distal perception implies a progressive outlook on research design. Both the distal and the motivational—that is, central—emphasis in perception psychology are new developments. Each in its own coldly cognitive or more hotly soul-searching way is a constructive protest against the sterility of peripheralism, whether that of nineteenth-century structuralism or that of the earlier stages of behaviorism.

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NOTES TOWARD A PSYCHOLOGICAL THEORY[1]

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The word "Notes" in the above title is not a reasoned and sly attempt to appear properly modest, nor is the phrase "Psychological Theory" meant to indicate anything short of all that should be implied by such a grandiloquent phrase. To comment upon each of these in reverse order: I propose, in this paper, to argue that up to now we have had no explicitly stated *complete* psychological theory, but rather, that we seem to have contented ourselves with a collection of discontinuous, unrelated "theorettes." It is my opinion that psychology has now reached the stage where it has accumulated sufficient data and understanding to reformulate, explicitly, its entire theoretical framework. And I propose to point to the proper direction in which the theoretician in psychology should move. But at the same time I will not go beyond such a general pointing. I have only a few notes to offer. The specific contents of what I have to say can be, at best, only programmatic in nature. This is so simply because I am at present incapable of doing a more meaty job. I fully appreciate that like all programmatic statements this paper will run the danger of sounding as though the statement of the program itself is to be conceived as part of the job it calls for. I should therefore like, here and now, completely to disavow such intent. However, I am willing to risk this danger because I believe these notes point to an important direction and because I feel that this program, in the hands of abler theoreticians and experimenters, will prove extremely fruitful.

Some General Comments (Diatribe and Apologia)

The "compleat psychologist" like Walton's "compleat angler" is pretty much of a fish story—a pleasant daydream of a nonexistent expert. We have learning psychologists, perception psychologists, and motivational psychologists—and as many other kinds of psychologists as there are possible ways of classifying psychological data. This is true not only of the empiricists among us, who perforce are limited to rather sharply defined problems; but it is equally true of the systematists among us, who operate on the cosmic level. For corresponding to every conceivable way of classifying the data of psychology we have erected separate and independent theories, systems, and approaches—but we have no single and unified set of principles and laws which encompass *all* of psychology. Were one to aspire to the role of the "compleat psychologist" and to set about the writing of a serious theoretical treatise covering all of behavior, his output would have many of the characteristics of the confused delusions of hebephrenia—to use a term suggested by Gardner Murphy (4). In discussing one sample of behavior he would have recourse to "learning principles," another sample of behavior would necessitate his calling upon "perceptual principles," a third, "motivational principles." *And the truth or falsity of any one of these sets of "principles" would not be dependent upon the truth or falsity of any other set.* The tome of our would-be "compleat psychologist" would, in essence, consist of three different books—digesting the work and thinking of three different sciences concerned with the study of three different kinds of organisms (none of which, I am prepared to argue, actually exists). This would be so because our mythical aspirant would discover that traditionally most psychologists have so answered the question: "What are the categories of psychological data?" as to create three "basic" categories: learning data, perceptual data, and motivational data—each with its own set of independent principles and laws. He would find that many of our experimenters and theorists have concerned themselves with nonperceiving, nonmotivated LEARNERS; with nonlearning, nonmotivated PERCEIVERS; with nonlearning, nonperceiving NEED-STRUCTURES. He would seek in vain for a set of descriptive terms which would enable him even to guess at the nature of an all-encompassing unitary set of principles.[2]

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[1] I wish here to express my appreciation to Professor Donald MacKinnon, whose critical interest in this paper had both encouraged my completion and publication of the manuscript and has prevented a too hasty solution of some of the issues I raise here.

But perhaps, one might argue, the world can best be described in terms of the confused delusions of the hebephrenic. Why must we assume that nature is cast in the pattern of the systematized delusions of the paranoiac (to borrow another description from Murphy)—a pattern which is obviously the one I would defend and seek? Perhaps the truth of the matter is that there are different basic categories of data and correspondingly different sets of principles and laws. Why must there be only one grand set of cosmic laws equally applicable to all experience or behavior? My answers to such a query are a bit too many for my own comfort since I have always felt that if one has more than one reason for a choice, every reason is suspect. However, let me discuss three of these reasons briefly.

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[2] The recent concern with the need for "integrating" the work of learning, perceptual, and motivational psychologists will be discussed later in this paper.

In the first place I find a reason in the lessons taught by the history of science. Other sciences have found that scientific inquiry frequently becomes more fruitful in almost direct proportion to the process of unification of principles. The most dramatic instances of this are, of course, the recent developments in physics. Someone has pointed out that a study of the physics textbooks over the last several decades has shown that as time goes on, data and knowledge in the field accumulate at a rapid rate, but the textbooks become slimmer and slimmer! The reason for this paradox seems to be that as understanding increases, the different number of principles and laws necessary to encompass the data *decreases*. The separate laws of matter and energy, of motion and gravitation are set aside for a single formulation, for a single set of laws which covers all of these phenomena. "Fields" of physics disappear and "Physics" begins to appear. It is of more than historical interest, in this connection, to note that Einstein has written in his autobiography[3] about his view of physics when he was a student. (In reading the following excerpt it might be instructive, or at least amusing, to read the word *psychology* for the word *physics*):

> . . . physics . . . was divided into separate fields, each of which was capable of devouring a short lifetime of work without having satisfied the hunger for deeper knowledge. The mass of experientially given and of the insufficiently related was overwhelming.

My second reason derives from my notion of what science is

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[3] Quoted from an excerpt printed in the New York *Times*, Sunday, March 13 1949, p. 34, prior to the publication of his autobiography.

I would maintain that the search for all-encompassing laws is *demanded* of any good citizen of science. The "scientific way" is a reductive way. I doubt that any scientist would maintain that one is behaving like a scientist when he finds it necessary to draw upon a different and independent "law" to explain every separate observation—where his system requires as many "laws" as he has raw observations. Everyone would agree that it is the business of the scientist to explain a set of n *events* in terms of a set of $n-\Delta$ principles. I prefer to be consistent and further to assume that it is the business of science to group a set of $n-\Delta$ *principles* under an even smaller set of more embracing principles, and so on *ad infinitum*.

I have another reason. In all truth I must admit that my preference for the cosmic, all-inclusive, unitary principle is just that—a *preference*. I prefer to believe that there is a simple (but not simple-minded) order to the universe. This no doubt expresses some sort of personality trait, quirk, idiosyncracy or defect, or perhaps merely my notion of the good, the true, the beautiful.

A Fundamental Question of All Psychology[4]

The picture I have drawn of the learning psychologist, perception psychologist, and motivational psychologist, each working only one side of nature's street in splendid isolation and each refraining from getting involved in jurisdictional disputes with one another, has more historical than contemporary validity. As I have already indicated, many psychologists are now beginning to see the "interrelatedness" of each other's data. It is becoming increasingly clear that the "basic" categories cannot be defended as being fixed and mutually exclusive and therefore many of us are beginning to question the adequacy of the various sets of principles based upon such categories.

In response to this ambiguous and uncomfortable situation a number of psychologists have sought various ways of resolving the problem. I should like to discuss three of these approaches. The first might be termed the "parliamentary and individual sovereignty" approach. This approach calls for a meeting of the rival theorists around a table and talking it over—each prepared to give and take a few concessions. The perception psychologist, thus, could concede a certain degree of motivational influence in perception and write that "needs are among the determining factors in perception." The need psychologist could admit to a modest amount of cognitive regulation of motivation and write that "learning modifies needs"; and the learning psychologist could cheerfully recognize that learning involves perceptual reorganization as well as motives and write that "learning is a function, among other things, of perception and motivation."

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[4] I wish to express my debt to Professor Robert B. MacLeod, with whom I have discussed some of the material in this section and from whom I am borrowing liberally in the present formulation.

The second approach might be termed the "imperialistic" approach (to continue our game) and would consist of the perception psychologist's attempt to demonstrate that all learning phenomena and all motivational phenomena are merely special instances of perceptual reorganizations; the learning psychologist would attempt to show that all perceptions and needs are merely conditioned responses or learned reactions, or hypotheses, etc.; while the motivational psychologist would continually stress the "dynamic" aspects of so-called perception and so-called learning and might perhaps even dispose of these data as merely a partial or largely illusory expression of the deeper motives of man.

Both of the above approaches have some value, but neither has enough. The first of these—the give-and-take approach—would usher in an era of good feeling and of continued sterility. The second of these—the imperialistic one—would draw the battle lines more sharply, would make for a devil of a lot of fun and recriminations, but would also add very little of permanent value. What is required is not the kind of give-and-take that leaves the givers and takers in essentially unchanged positions with their salvaged "basic" categories, nor the kind of patchwork and worried working-over of perceptual or learning or motivational laws which attempts to force "other" data into basically inadequate formulations. What is required is a third approach (one which might be called the "revolutionary" one) which is prepared to make a completely fresh re-examination of the whole business of "categories" of psychological data, an approach which will, in so far as possible, avoid the crippling effects of our trained incapacity in looking at the phenomena which are basic to *all* of psychology.

The task is, in the first instance, one of description and classification. The existing categories of perception, learning, and motivation represent one method of classifying the data of psychology. In re-examining these categories, the inquirer must answer three questions: (1) What are the data to be classified? (2) What are the characteristics of these data which in the past have sustained the traditional classification? (3) Do these data, when freshly inspected, suggest different and better methods of classification?

To simplify matters let us assume that most psychologists have agreed that there are two alternative answers to the question: "What are the data of psychology?" These are represented respectively by the psychology of act and the psychology of content. In more modern language these alternatives of Titchener are a psychology which takes as its point of departure a "biological organism" (behavior) and a psychology which is concerned with the organization and structure of the psychological field (experience). This question of what constitutes the fundamental data of psychology is basic to all psychology and one to which no universally satisfying answer has been found. In spite of the aesthetic unpleasantness (to me) of the following solution, I should like to suggest that it is possible to hold on to each of the above alternative interpretations, and in the light of *each* to attempt to answer the two succeeding questions of methods of classifying the "data." I am, in other words, laying the first question "on the table" for later discussion and am proposing that we can, nevertheless, fruitfully go ahead with a discussion of "basic categories."[5]

Even a cursory glance at history reveals the unsystematic way in which the basic categories of psychology have been set and maintained, quite irrespective of what has been assumed to be the data of psychology. For Plato the ultimate values, truth, beauty, and goodness, dictated a threefold classification of psychological processes, designated in more modern language as the cognitive (knowing), affective (feeling), and conative (willing) aspects of personality. Aristotle likewise recognized three levels of psychological organization: the nutritive, the sensitive, and the rational (read: motivation, perception, and cognition or learning). The medieval Church tended to simplify the Greek trichotomies into a dichotomy of the rational and the irrational (read: reason and emotion). The point I should like to make is that the historically minded scholar could easily support the thesis that modern psychology is a victim of folklore concepts, that our "modern" categories represent not a scientifically determined and defensible system, but a happenstance one. In a sense *we have been trapped by the language of the common man.*

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[5] Hochberg and Gleitman (2) suggest (and illustrate) an interesting possible resolution of this dichotomy. They propose a third alternative in which "perceptual" equilibration and "motoric" equilibration are both considered as *part-processes* in a larger organization whose radix, in human beings at least, is predominantly on the perceptual side. They point out that while the organizational characteristics of the "motoric" process (i.e., a "part-process" in the larger organization) may not be represented in experience, it is a useful first approximation to assume a continuity and similarity between the two part-processes.

I should like to enlarge upon this a bit. Probably most of us would admit that the words of man reflect to a considerable degree the experiences and behavior of man. But at the same time I would urge that the concepts of science need not (and indeed, should not) reflect the words of man. It is an indefensible notion, to me, that for every word common to our culture, science should have a corresponding "basic" concept or category of data. Other sciences have not avoided this trap, psychology least of all. For example, the fact that in our culture most of us refer to different conscious experiences or different observable behavior patterns when we use such words as "propaganda," "persuasion," "education," "suggestion," does not mean that behind these words, these experiences, and these behaviors there are four basically different processes, or that we must discover or invent four basically different hypothetical constructs as correlates of these four different experiences. All these different experiences and behaviors may represent variations along one or more dimensions of *one* process or *one* construct. The chemist does not have different constructs for an iron frying pan, an iron kettle, and an iron hammer. He is content with the construct of "iron"—and yet a frying pan is quite different from either a kettle or a hammer. We must constantly remember that the phenomenological experience and behavior of man is important, but important as a *starting point*, not as an end point. Our concepts must be adequate enough to permit us to encompass all experience and all behavior, but our concepts cannot be as pluralistic as our experiences and behavior patterns.

Certain it is, given our cultural background, our language, and our folklore, that the words *perception*, *learning*, and *motivation* make sense to most of us. But that is no indication that there therefore must be three basic categories of psychological data or constructs or processes called "perceptual," "learning," or "motivational." Actually this distinction will probably not hold up even on the basis of a careful phenomenological investigation. The really naïve person—unadulterated by the speculations and theologies of the Greeks and the medieval Church—when asked to describe his experience in an "emotional" situation would probably *not* limit himself to an account of heart-flutterings, pulse rates, sweating, etc. He probably would include a great deal of "perceptual" material, "cognitive" material, "motivational" material, etc. He would tell us how things *looked* to him, what things he *remembered*, what plans for escape he was formulating, what he *wanted* to do, etc. The same would be true of his so-called "perceptual" or "learning" experiences.

How many of these distinctions—perception, learning, motivation—would remain if we temporarily forgot our history and folklore and simply inspected our data? I am suggesting, on the basis of the recent experimental work of the "need-influencing-perception" school, that none of them would remain. I am of the opinion that such an inspection would give us a set of categories which would *cut across* all three of these (as these are understood today). I wish now to plunge right in and indicate the nature of this different and "better" method of classification of psychological data.

A Tentative Reformulation

The third approach which I have indicated above (the "revolutionary" one) has already been attempted at some points, and with significantly promising results. Before I outline my own suggestions, therefore, it might prove of value to consider some of those attempts, especially since they are congruent with what I wish to propose. I should like to refer particularly to Lewin's concept of *Aufforderungscharakter* (valence) and Tolman's *Sign-Gestalt* theory.

As I understand Lewin's use of the concept of valence, it is more than an assertion that needs influence our perception of an object.

The *perception itself*, under certain conditions, has invitation character, and this invitation character is as much a genuine perceptual attribute as is color or shape. It is not a question of gluing on a "plus" sign to a "perception," but the very perceptual experience is demanding or rejecting. It would therefore be a hopeless and fruitless task to attempt to discriminate between the purely "perceptual" and purely "motivational" aspects of the experience resulting from the stimulation by a valenced object. In this one instance, in any event, Lewin has discarded the "basic" categories of perception and motivation and has created an entirely new category.

Tolman, in his insistence that we never experience *gestalten*, but always *sign-gestalten*, does violence to the two categories of "learning" and "perception." It is not a question, he would assert, of learning *influencing* our perception of a stimulus. We never experience a pure "perception" devoid of its functional value. Our basic, immediate experiences resulting from any stimulus are *sign-gestalten* (see especially his "Gestalt and Sign-Gestalt," **6**). Again, with the sign-gestalt concept in mind, it would be a futile task to attempt to discriminate between the purely "perceptual" and purely "learned" aspects of experience.

Perhaps a major reason for Lewin's and Tolman's ability to get away from the old basic categories lies in their willingness to use "hypothetical constructs" in their thinking. And it is in the use of this powerful tool of hypothetical constructs that I think lie the secret of much of the genius of the modern scientific theorists and also the greatest promise for future psychological theory. Since most of what I have to say in the succeeding sections of this paper is dependent upon the use of such intervening structures, I should like to spell out the way in which I intend to use them.

Hypothetical Constructs

In recent years a great deal has been written about the status of hypothetical constructs in psychology, and I intend to use the term as defined by MacCorquodale and Meehl (**3**). A hypothetical construct refers to a postulated actually existing structure which might eventually be described by direct experimentation. As such it is centrally located, and in the sequential history of stimulus-response the functions of the hypothetical construct intervene between stimulus and response. A hypothetical construct, further, will have properties beyond those functional equations through which it was first described. Thus, "atoms," "genes," "neural traces" are instances of hypothetical constructs. Each of these is assumed to have existence, and it is hoped that each of these may eventually be described by more direct experimentation and observation than we are capable of at the present.[6] Each of these has been ascribed properties beyond the originally observed experimental data which led to the postulation of their existence. Because it is assumed that these hypothetical constructs exist, and because of the intrinsic properties that they are assumed to have, the correlations between experimental conditions and results are now seen as *necessary* correlations, as inevitable consequences of the functioning of these hypothetical constructs.

A number of psychological theories make use of hypothetical constructs. In this group would fall the psychoanalytic theories, the Gestalt theories, and much of Tolman's theorizing.[7] All these theories are full of hypothetical constructs—sometimes very vaguely and unsatisfactorily defined, sometimes more clearly defined.

"Dynamic Systems"

In the following discussion I shall postulate the existence of a hypothetical construct which I will dub with the very unsatisfactory name of "Dynamic System." This Dynamic System is to be understood as an actually existing structure with intrinsic properties of its own. I wish to repeat: in my thinking, a Dynamic System (which, of course, I have never seen or measured nor do I expect ever to see one) should eventually achieve (if it is to be of value) the same reality and status as the "positron" or "neutron" in physics and the "neural trace" in neural physiology.

The Dynamic System is assumed to be isomorphically related to a system of neural traces. Dynamic Systems are the major units of our psychological fields and are the resultants of every neural event which is reflected in any kind of brain event. They are the resultants, in other words, of the neural traces remaining from past brain activity, from stimulation originating outside the organism or inside the organism, and they are conditioned by the anatomical and constitutional factors of the organism. The kind and number of Dynamic Systems, the changes which occur within each Dynamic System, and the interaction among Dynamic Systems determine all experience and behavior. In a word, to understand or predict correlations between stimulus conditions and reactions, we must know all about these Dynamic Systems.

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[6] Recent work in genetics has already suggested the very strong possibility that we may soon be able to "see" the gene.

[7] It should be pointed out, however, that Tolman has been somewhat inconsistent in this respect. At some points he writes as if he is operating with "intervening variables," at other times, with "hypothetical constructs."

If we are to make any meaningful use of a Dynamic System, it becomes essential to define and describe, in great and specific detail, the intrinsic characteristics of the Dynamic System as well as its laws of operation. It is necessary, in other words, to build specific, concrete models. From a programmatic point of view this task assumes highest priority. And, to repeat what we have already indicated, this game of model building is one which other sciences have played at and is a game which has "paid off." Of course there are certain rules which one must respect if one is to play this game. Among such rules the following one must always be kept in mind: The characteristics of the model must reflect experiment. Attributes are assigned to models only because they help explain available data and help predict new data. However, these attributes must go beyond the data; they cannot be, in other words, phenotypic in nature. For example, it would help us little to ascribe the attribute of "suggestibility" to a Dynamic System and then use that to "explain" the phenomenon of "suggestion" in behavior.

What is the model for a Dynamic System? What are its attributes? What are its laws of operation? I have already confessed that I do not know. This must yet be discovered. But I shall make a few sketchy assumptions—assumptions which derive, I believe, from data and experiment, and assumptions which are really identical with those already made by the Gestaltists in defining their own hypothetical constructs of "cognitive structures," "tension systems," etc. One might therefore raise the question: "How then is what you have to say any different from what Köhler and Koffka and Lewin have already said so frequently and so well?"

To the reader familiar with the Gestalt literature the following discussion will probably evoke in him an experience of *déja vu* (no matter how bizarre and distorted the image will appear to him). There are obvious reasons, as I have already indicated, why this will be so. Almost everything I shall have to say will have been borrowed from the Gestaltists' field approach and from their "organizational principles." But herein lies my immediate objective: I am attempting to draw out, explicitly, what is already involved implicitly in their theorizing. I will attempt to generalize their organizational principles to behavior units different from those for which those principles were originally invented. I find it necessary to do this because I believe that the Gestaltists themselves have failed to follow through on their own suggestions with the result that they have failed to recognize, *in their experimental work*, the unity of the psychological field which is made possible by an *explicit* statement of their own theory. They have been inadequate Gestaltists. The very term "cognitive structure" indicates or implies that their hypothetical construct refers to the "perceptual" or "cognitive" aspects of behavior. How they would deal with the "motivational" aspects is not clearly and explicitly formulated. Actually, most of their experimental work has neglected (except for Lewin's work) this "aspect" of behavior altogether. And yet, as I shall attempt to show, by spelling out, explicitly, the significance of the attributes which they have already ascribed to their "cognitive structure," the problem of "motivation" can be dealt with quite easily and elegantly. However, it should be further noted, that in doing this, we will find it necessary to drop "perception" as a basic and autonomously existing process. I am not advocating, in other words, the "imperialistic" solution.

I shall limit myself to borrowing three of their assumptions. I shall assert that Dynamic Systems differ in "Isolation," "Differentiation," and "Tension." By *Isolation* I refer to the assumption that Dynamic Systems differ with respect to the degree and kind of interaction possible with other Dynamic Systems. One can speculate (as Lewin has) as to whether this characteristic is a function of "boundary permeability" or (as other Gestaltists have) of "similarity." These are important questions which I leave completely unanswered. By *Differentiation* I refer to the assumption that Dynamic Systems differ in the number, kind, and interrelatedness of the substructures of the system. Every Dynamic System, it is assumed, is analyzable into subsystems, but some of the Dynamic Systems are more richly subdivided than others and some are more easily restructured in their divisions than are others. By *Tension* I refer to the assumption that each Dynamic System is so organized as to vary along a dimension of equilibrium. Certain organizations of the forces within a Dynamic System may result, for instance, in such instabilities as to initiate a process of rapid or extensive reorganizations. Other Dynamic Systems may have such a distribution of forces as to present a minimum state of disequilibrium with the result that very little "spontaneous" reorganization would go on within the Dynamic System. So much for the attributes of Isolation, Differentiation, and Tension.

As I have already pointed out, I assume that *every* experience or behavior of man is correlated with, or is determined by, or is congruent with, or is a function of a Dynamic System. Thus, when our subject is told to fixate a figure on a screen, we postulate, for the viewer, a corresponding Dynamic System. This Dynamic System will be a product of many influences—of the neural activity consequent upon the retinal stimulation by the wave lengths emanating from the figure, of the neural activities consequent upon the spoken instructions of the experimenter, of the neural activity consequent upon the subject's "seeing" of the experimenter as such-and-such an individual, of the neural activities consequent upon a selected series of glandular and muscular activity, of the neural activity consequent on the activity of a selected series of neural trace (memorial) activities, etc. Much the same story will be true for the Dynamic System which will come into being, or which will correspond to the experience of a man who reads a piece of propaganda, sees a steak, "feels" hungry. *Each* of these Dynamic Systems, it must be emphasized, will vary along the dimensions of Isolation, Differentiation, and Tension.

I have already stated that Dynamic Systems frequently (although not always) result in conscious experience. These conscious correlates can be conceived, I would assert, to be a function of the basic attributes of a Dynamic System. Let me take one of these as an illustration. Variations of the attribute Tension will contribute to the conscious correlate of a "feeling of restlessness," "desire," "need," "demand," etc. (I would invite the reader to pick any phenomenological descriptive term he wishes). Now I take it that these particular conscious correlates are the aspects of experience to which most of us refer when we speak about "motivation." *But,* from everything I have assumed about Dynamic Systems, Tension is a characteristic of *every* Dynamic System and, further, the conscious correlates will also reflect the Isolation and Differentiation characteristics of the Dynamic System. The implications of these facts are as follows: (1) "Motivational" factors or attributes will be involved in every one of our experiences and can derive from almost any stimulating situation. That is, a Dynamic System which is co-ordinate with our experience when we look at a "lopsided" figure on a screen will be *under the very same kind of tension* as a Dynamic System which is co-ordinated with our experience when we look at a steak after several hours of food deprivation. There is no "unmotivated" situation. (2) The so-called "motivational" attributes of any experience will be intimately co-ordinated with all the other attributes of the particular Dynamic System in question. There can be no "pure" hunger motive, or "sex" motive. If we wish to speak of a "hunger motive," we must simultaneously speak of the "perceptual," "memorial," and other processes involved—or better still of the Isolation characteristics and Differentiation characteristics of the particular Dynamic System. "No hunger without hunger for something specific" is the motto. My "hunger" is not the same as yours, because my Dynamic System *re* food and eating is not the same as yours.

Let me take another illustration. That behavior which most of us refer to as "learning" is co-ordinated with reorganizations of the Dynamic System involved.[8] As the high-school student learns to recite the theorem about the sum of the angles of a triangle, the Dynamic System co-ordinated to triangles for this student undergoes many reorganizations from his first lesson to his last. But, obviously, much more is changed in his Dynamic System than the characteristic of Differentiation. The characteristics of *Isolation* and *Tension* are also changed—by very definition of what constitutes a *Dynamic* System. To use the layman's language, as the student learns about triangles, his *knowledge* about triangles changes, his *perception* of triangles changes, his *interest* (or motives) in triangles changes, whether you define "knowledge, perception, interests" in behavorial or phenomenological terms.

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[8] This formulation, it will readily be seen, is very similar to the one suggested by D. K. Adams (1) many years ago.

All of the above is another way of repeating, of course, what I have already indicated several times. There can be no laws relating to "learning," to "perception," to "motivation." The laws concerning the functioning of Dynamic Systems must involve *all the parameters of Dynamic Systems simultaneously.* And since experience and behavior are controlled by the functioning of Dynamic Systems, the only laws which enable us to understand and predict behavior must be laws of Dynamic Systems—not laws relating to "Isolation," "Differentiation," "Tension," etc. Experimentally this means that we cannot talk about "varying the stimulus conditions and holding motivation constant," or "varying motivation and holding knowledge constant." Varying the stimulus conditions will vary the Dynamic System and therefore *all* of its attributes. Of course we can still analyze behavior, and we can still vary experimental conditions, and we can still observe correlations between experimental conditions and behavior—these operations are basic to experimentation—but our categories of behavior will not be in terms of "perception," "learning," and "motives"; and our descriptions of the experimental conditions will not follow our present pattern of partiality in description. I anticipated three major changes—in our *unit of behavior or of experience,* in our *descriptive categories* of behavior, and in the description of our *experimental conditions.*

Our unit of behavior or of experience, no matter how small, will always involve "perception-need-feeling-belief-emotion-etc." Our descriptive categories will probably be in such terms as "rigidity," "complexity," intensity," etc. I am here guessing at the nature of more useful descriptive categories than we now have. Our description of the experimental conditions will take into account what the subject "sees," what the subject "remembers," what the subject "wants," etc. And all this will hold whether we are studying behavior in response to india-ink drawings or behavior in response to twenty-four hours of food deprivation, or behavior in response to a radio broadcast. Again, to use the Gestalt experimentalist as our whipping-boy (probably because I am beholden so much to them): I have elsewhere in this paper accused the Gestaltists (by implication) of having operated in their laboratory and in their thinking as if they could really work with "unmotivated perceivers." This accusation I believe to be well-founded. Because they have refused to pay attention to the data and observations of the clinician, the personologist, the social psychologist, etc., much of their work suffers from inadequate descriptions of their experimental situations and from artificially created molecular units of analysis. Rarely does one find in a Gestalt experiment any description of the "personality" of the perceiver, his relation to the experimenter, his over-all view of the experimental situation in which the experimenter has placed him. From everything I have tried to say it should be apparent that to describe the experimental situation only in terms of a small segment of the external field (i.e., the nature and conditions of illumination and the size and shape of the india-ink drawing) is hardly sufficient. To analyze, then, the resulting "*cognitive* structure" is to analyze an artificially created sliver of the hypothetical construct which must intervene between stimulus-field and response. It is for these reasons that I believe that many of their specific laboratory investigations must be redone and many of their specific "laws" revised. And, of course, so must much of the work of the learning and motivational psychologists be redone.

Some Implications and Problem Reformulations

So much for the more "formal" outline of my suggested approach. The reader will perhaps be able to judge better the value of this approach if instead of elaborating on the above in generalized terms, I attempt to present some of the implications of this approach for the reformulation of some of the traditional problems and researches in psychology—suggesting, where possible, *experimental* implications. Perhaps in that more familiar context the strengths and weaknesses of this approach can be more clearly assessed.

The Relationship between the "Cognitive" and the "Motivational" Life of Man

This is a very thorny problem in the side of the psychologist. It is phrased in many ways: "Is man rational or emotional?" "Is there a correlation between knowledge and attitudes?" "Does man respond with his cortex or his guts?" I should like to suggest that what has happened is something like the following: The early Western philosophers, or the Church, or Western folklore split asunder what nature had put together and spoke of "cognition" and "emotion" as two basically separate functions or processes. The modern psychologist, on the basis of his clinical observations and experimental data has discovered that the "two" are not independent. Instead of taking the bold step of exploring the possibility that we are not dealing with two processes, or two forces, or two events, or two structures, he has continued to think in terms of this dichotomy and seeks the answer in studying the *interaction* of these "two things." For all their talk about the "unity" of behavior, these interactionists are still "separatists" (to use Hochberg's and Gleitman's term, 2). The approach suggested in this paper urges an entirely different solution. There is no Dynamic System without Tension *and* Differentiation *and* Isolation, and therefore there can be no behavior which is not at one and the same time "cognitive" and "emotional." Thus the problem as it is usually phrased ("Is man rational or emotional?") is seen as a pseudo problem and as essentially a nonsensical question. But these questions *do* refer to some interesting and important behavioral problems. Our theoretical reformulation does not "reformulate out" those problems. Instead it suggests posing the following experimental and theoretical questions: "Why does some behavior depart radically from reality, while other behavior does not?" "What are the determinants in the psychological field for this difference?" "Why are some Dynamic Systems more rigid than others?" "Why are some Dynamic Systems more isolated from the rest of the individual's Dynamic Systems than are others?" A little thought will make clear, it seems to me, that most of the problems usually phrased as "rational versus emotional" will be covered by this sort of formulation and experimentation, and, it is suggested, with greater promise of fundamental and meaningful solution.

"Basic" versus *"Derived"* *Needs*

Here, too, is a perennial problem which has not been solved to anyone's satisfaction. "Are there universal and basic needs?" "Are biological needs different from social needs?" "Are social needs dependent upon tissue needs, or are they functionally autonomous?" The answer which the present approach would give would run something like this: Since every Dynamic System varies along the dimension of Tension as well as other dimensions, and since the degree or amount of Tension is a function of *all* the events determining the Dynamic System, we must look for the cause of behavior *initiation* in the characteristics of all the determinants of the Dynamic System—in neural activity resulting from sources *outside* the organism as well as those from within the skin of the organism. In other words, we look to the total field for the forces which *initiate* behavior, i.e., for "motivation." We no longer have to say, apologetically and in footnotes, "Of course, maybe in the dim future we shall find a 'tissue need' behind the need for dominance, or the need for beauty, or any of the other secondary and social needs."

The Dynamic System resulting from our being exposed to a picture hung askew on a wall, or our hearing but one shoe dropped in the room above, or our seeing an advertisement of a new automobile may be as full of tension and *is of the very same kind of tension* as is the Dynamic System resulting from food, or sex, or rest deprivation. An interesting example of this is provided in Torrey's experiment (7), which indicates that the tendency to resume interrupted tasks can be initiated by the structural characteristics of the external stimulus as efficiently as it can by any "ego involvement."[9] This entire theoretical approach insistently demands that we look to the entire field for our understanding of experience and behavior—its style, its content, *and* initiation.

The Relation between Learning and Motivation

"Is learning without motivation possible?" "Is latent learning latent learning?" "Does increased motivation speed up learning?" Here again the present theoretical approach would demand a complete rephrasing of the experimental questions *and* experimental procedures. In the first place, as the reader can well testify, we have pointed out, *ad nauseum*, that every Dynamic System involves Tension, that "learning" consists of the reorganization of a Dynamic System, and that, therefore, there can be no reorganization without some change along the Tension dimension as well as along other dimensions. But we can say much more than that in the present connection. The traditional approaches to the learning-motivation problems have assumed that that which is learned is learned—no matter how efficiently, no matter under what kind of deprivation schedules. That is, it has been assumed that animals who learn a maze under conditions of 24 hours of food deprivation are learning the same thing which animals working under 48 hours of food deprivation learn. Of course the latter animals may learn more quickly (or sometimes more slowly), but when they have "met the criterion" they have learned the *same* thing. But in our present terms, such an assumption is impossible. Since the Dynamic System of each of these animals is not only a function of the external stimulus field (the maze, etc.) but *also* of the internal stimulus field (stomach contractions or whatnot), then the two resulting Dynamic Systems must necessarily be *different.* Each of these groups of animals has "learned" something different. Actually we already have some experimental evidence for this supposition—a supposition demanded by our approach. The "cognitive map" (to use Tolman's term) which results from an animal on one schedule of deprivation is different from that of an animal on a different schedule of deprivation. One animal's maze habit is more "rigid," another's more "docile," etc. O'Kelley's and Heyer's rats (5) trained under conditions of 36-hour deprivation are superior in *retention* to their rats trained under a 35-hour deprivation with two thirds of water intake before their runs, although no differences were observed in the original learning efficiency of these two groups. We would argue that the Dynamic Systems resulting from these two different experimental conditions being different, the "fate" over time of these two Dynamic Systems would also have to be different.

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[9] Torrey's experiment, incidentally, might be cited as one indication of the experimental fruitfulness of the present theoretical orientation.

Our present approach suggests many other experimental problems. Since the "speed of learning" will be a function of the lack of equilibrium in the Dynamic System, and since that in turn is a function of all the characteristics of a Dynamic System, we can "speed up" learning by manipulating the nature of the problem situation as well as the nature of the deprivation situation; covariant combinations of the two will yield predictable results. For example:

Presenting an animal, under conditions of minimum deprivation, with a stimulus situation high in "tension" might lead to just as rapid learning as presenting an animal, under conditions of increased deprivation, with a stimulus condition low in "tension." Instead of repeatedly and monotonously asking the question: "How does so-and-so much deprivation affect speed of learning?" we would ask the question, "How does so-and-so much deprivation in such and such a problem situation affect *what* is learned and how quickly is it learned?" We would never assume "all other things being constant" either in the experimental situations or the resulting "behaviors."

Psychotherapy

Rephrased in the present terms, the problem of psychotherapy is seen as a problem in the re-education of the entire individual. It is not seen as a problem of changing the patient's "needs" or "need-fulfilment techniques" or as one of resolving "conflicting motives." It is seen, in our terms, as a problem of reorganizing the major Dynamic Systems of the patient—and all that that implies. It means changing the individual's "perceptions-needs-belief-attitudes-feelings-emotions" complex—to use everyday language. It means changing his way of thinking, his problem-solving techniques, his figure-ground perceptions, his memory, and almost everything else about him. Actually, of course, that is the position which has been maintained by many psychotherapists. But one gets the impression that despite the enunciation of this principle again and again the psychotherapist, either because he lacks a proper theoretical framework within which he can apply this system or because of some tendency to become too enamoured of manipulating "basic" needs, fails to take his own statements very seriously. Whatever may be the situation, I am suggesting that the present theoretical approach would tend to give the same answer to the problem of psychotherapy which the psychotherapist himself feels is required on the basis of his clinical experience. In so far as it does that, we are here providing a theoretical framework for the *research* psychologist which should at the same time prove useful for the applied psychologist. Because this theoretical approach insists that *all* psychologists must work with units of analysis which do not do violence to the fundamental unity of behavior, there is the hope that the laws of Dynamic Systems (presumably to be discovered by the research psychologist) will encompass the data of the clinician and, in turn, will be more immediately applicable to the work of the psychotherapist than is true of our present "psychological laws." There is no more pathetic sight in modern psychology than the one provided by the academic psychologist's attempt to demonstrate that his theoretical formulations have something substantial to contribute to the clinician. This is especially obvious in our graduate schools, where the "clinical student" is forced to learn "fundamental psychology" because, he is told (and usually by a professor who really doesn't believe what he is saying) it will "help him" in his clinical thinking, research, and work.

A Final Comment

As I was quick to confess at the very beginning of this paper, I have here presented only a suggestion for a program of theoretical work—work which is yet to be done. Little can be gained, therefore, by attempting to bolster my present argument by further speculation, translation, and vague experimental suggestions. If the reader cannot now see any value in the approach I have outlined, so be it, for I have no additional arguments to present at this time. My own hunch is, however, that the theoretical work which must soon follow from the current experimental work coming out of the laboratories of the "New Look" adherents[10] will proceed somewhat along the lines indicated above. It is because I think that many psychologists might be ready to consider this approach sympathetically and creatively that I have been prompted to write up this series of notes. But before leaving this paper I wish to make one final "explanation."

Throughout this paper I have consistently absolved Kurt Lewin from the sins I have ascribed to the other Gestaltists. He, above all of his co-workers, did pay attention to the data and observations of the clinicians and the personologists and the social psychologists. His system can hardly be described as a purely "cognitive" one. Wherein, then, does the approach I have presented differ from his?

[10] I refer, by this phrase, to such work in perception as that of Jerome Bruner, Gardner Murphy, George Klein, Heinz Werner, David McClelland, Leo Postman, Herman Witkin, Mugafer Sherif, Henry Gleitman, Julian Hochberg, Else Frenkel-Brunswik, and many others.

I would suggest that it differs at several points. In the first place, whatever the merits of his system may be, in his actual *experimental use* of his theory, there seems to be a one-sided emphasis on the "motivational" aspects of experience and behavior. His major concern seems to be with the force relationships *among* various structures in the psychological field. Some of his structural units in his psychological field seem to be *contentless tension systems*, others seem to be tensionless content-systems, and behavior seems to be a result of the "forces" between these separate structures. Thus, a valenced object is conceived by him as a specific structure with sign which requires a "tension system" *elsewhere* in the psychological field before the organism will locomote toward the object. What this tension system is, is not clear. Frequently he writes as if this "tension system" is merely another word for the concept of "need" or "motive," but frequently he writes as if this is not so. Thus, for example, in a "conflict" situation, the "person" is pictured as being in "tension" and the two perceived goal-objects as lying outside of the "person" and as having their own and independent signs. What I should prefer, as is obvious, is one "closed system" structure with such characteristics and attributes as to make really inseparable the "tension of the person," the "valence of the object," the "distance separating the person and the object," and the "regions" through which the "person" must "locomote" in order to "achieve" the "object."

But whatever the differences and similarities between Kurt Lewin's more defined and more elaborated theory and my present vague approach toward a theory, it is quite obvious that I am especially indebted to the brilliance and creativity of Kurt Lewin.

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SENSORY-TONIC FIELD THEORY OF PERCEPTION

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Necessity for a Unified Theory of Perception

Theories of perception have evolved, have been revised and changed—as in other areas of science—concomitantly with expansion of knowledge and inclusion of new areas of research. Thus at the time when perception was dominated by investigations in the field of sensation, as in classical psychophysics, the predominant theory of perception was a sensory theory. With the advent of behaviorism and functionalism in psychology, a number of theories have been developed which emphasize the motor side of behavior, including perception.

It is also characteristic that in a widely accepted theory a number of facts might not be covered because they are considered peripheral to the main body of explainable data. These so-called peripheral facts through a shift of emphasis may suddenly become crucial and may force a revision of theory. Such a shift can be noted today with respect to the projective characteristics of perception. That perception may mirror emotions or the individual's motivations has of course been acknowledged for a long time within and outside of psychology. Experimental psychology looked on the problem of projective perception, because of its complexity and its experimental unmanageability, as unfit for scientific analysis. Though philosophers and theoretical psychologists have been intrigued by this problem for a long time, it was mainly the success of clinical psychologists using perception as a diagnostic tool which brought this problem of projective perception into the center of interest.

This shift, however, did not mean that the general theory of perception has been influenced immediately. A revision of theory demanded by new facts and change of emphasis is a painfully slow process. Though unity of science is posed by most of us as an ideal goal, in reality people are frequently satisfied with theories that encompass a limited and self-contained area. This deficiency is most glaring in the relation between clinical and general psychology. Psychologists who are mainly responsible for developing general theories of perception have been ignoring problems newly developed by clinical psychologists. On the other hand, the clinical psychologists have not seen the necessity for developing theories beyond the interpretational level on which they are operating.

Even where clinical psychologists who use perceptual tests have attempted to interpret their findings in general terms, they usually accepted an available theory in spite of its origin alien to the clinical facts. For instance, it can be shown that whenever Rorschach (**44**) discusses his findings in terms of general perceptual theory he uses atomistic and sensationalistic constructs in contradiction to his essentially organismic methodology.

General experimental psychology violates the principle of unity of science if its theories exclude facts of neighboring areas. As to clinical psychology, the restriction to its own level of interpretation may seem economical at first, but may show its deficiencies as soon as a fundamental grasp of perception-personality relationships is demanded.

To be sure, signs are not lacking that promise an eventual reconciliation of the divergent views of general and clinical psychology. During the last decade in America, the work concerned with the projective forms of perception has not been exclusively clinical and practical; theoretical psychologists are showing a growing interest in this problem. Using strictly controlled laboratory techniques, needs and motivation have been shown to affect perception.[1]

SENSORY AND MOTOR THEORIES OF PERCEPTION

Farthest removed from any theory of perception that would take care of its projective character are the sensory theories of classical psychophysics. If we concede that the analysis of the projective nature of perception can have a place only in a psychology that poses its problems in terms of personality-object relationships, then sensory theories are inadequate on two grounds. (1) The so-called objects with which the organism deals have no place except as derivatives of these hypostatized stimuli. (2) Classical psychophysics, as a prototype of sensory experimental methodology, considered as its methodological virtue its having ruled out the organism in its individual variability.

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[1] See, e.g., Sanford (**45**, **46**); Levine, Chein, and Murphy (**32**); Schafer and Murphy (**47**); Bruner and Goodman (**4**); Bruner and Postman (**5**); Brooklyn College Naval Research Project Progress Reports 1-8 (**3**). It may, however, be noted that perception as a projective means for theoretical analysis has a long history on the Continent, for example, the work of Heymans (**16**) and Wiersma (**63**) on "secondary function," Jaensch (**22**), the Kretschmer Group (**10**, **24**), etc.

From functionalistic psychology and behaviorism there have evolved theories of perception which try partly to replace sensory constructs by taking into account motor aspects of perceptual behavior. The central concept used by any motor theory of perception is that of interaction, i.e., interaction of sensory and motor factors. As long as this concept is used purely descriptively, one need not quarrel with it. However, if these theories offer interaction as an explanation of the changes in the perceptual field through motor activity, it immediately poses a paradox: How is it possible that intrinsically different elements, such as motor and sensory, can affect one another?

The paradox involved in a motor theory of perception that attempts to fabricate a unity out of alien elements can be likened to the attempts of the pre-gestaltists to unify the sensory elements into a gestalt. If it be conceded that it is possible to analyze perception into factors, such as motor and sensory, these factors cannot be considered to be elements the synthesis of which forms a perception. Logically the paradox can be solved only by postulating the priority of a whole with respect to such factors. In order to account for so-called interaction between motor and sensory factors we need the construct of a process which is prior to both.

The motor theories are doubtless superior to sensory theories in that they take into account the fact that there are other than sensory factors that constitute perception. They fail, first, because the crucial construct, interaction, as used by their theorists is paradoxical; and second, because the projective nature of perception cannot be treated adequately. The motor aspect in its strict sense is too narrow a construct to be representative of the total organismic activity that stands behind the notion of projection.

The criticism raised against the use of the concept of interaction is not meant to deny its scientific utility if properly defined. On the contrary we believe that no organismic theory can be developed without it. The conditions for interaction and the character of the interacting factors should take into account the following logical requirements: (1) Interaction presupposes priority of a total dynamic process which might be analyzable into its contributing factors, but which cannot be conceived as a summative result of these alien elements. (2) If factors are to interact, they must have common dynamic properties; or, in other words, they must be equivalent with respect to their contribution to the total dynamic process.

"Sensory-Tonic" Field Theory

Our theory assumes that perception is basically[2] a "sensory-tonic" event. This means that the factors contributing to perception are tonic as well as sensory. Tonus is used in its wide connotation. It includes the state of organismic tension as evidenced by the visceral as well as by the somatic (muscular-skeletal) reactivity. It refers to the dynamic (motion) and the static (posture) status of the organism.

Since interaction, i.e., interaction of sensory and tonic events is an essential feature of the theory, the previous statements concerning interaction are applied here to serve as basic postulates.

Thus perception is defined as a total dynamic process that can be analyzed empirically into its contributing factors that are tonic and sensory. But it should be noted that this end-process cannot be conceived as a mere combination of two factors which, considered singly, are alien to each other. Though different in terms of specific properties, they are dynamically equivalent; i.e., they must have dynamic properties in common which make it possible for them to contribute to the total dynamic process in an equivalent way.

The validity of the theory can be tested in terms of its two assertions: (1) that tonic factors interact with sensory factors in perceptual formation, and (2) that sensory and tonic factors are dynamically equivalent. Some evidence on these points will be presented later in the paper.

However, the "sensory-tonic" theory goes far beyond mere statements about interaction of sensory and tonic factors. The theory cannot be fully understood unless we relinquish the traditional notion of perception as a self-contained unit and replace it by a field concept which defines the relationship between organism and object. The potency of tonic factors for perception would be obscure without appreciating that the state of the organism is part and parcel of perceptual events.

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[2] Though it is recognized that perception has its cognitive and conative aspects, we are confining ourselves here to what we consider the primary level. The relation between this primary level and the more complex levels will be briefly discussed at the end of the paper.

The continuous interaction between organism and object requires the conception of a field with both body and object as constituents. We see that the above analysis of interaction must be applied again. First, the field of interaction has to be considered as a primary entity. Second, interaction can be assumed to occur only if the parts of the field, object and body, have common dynamic properties,[3] and in accordance with our theory are "sensory-tonic" in nature.

Within this general framework of "sensory-tonic" theory the following topics will be discussed: neurophysiological conceptions as they pertain to the "sensory-tonic" theory, evidence for sensory and tonic interaction, evidence for sensory and tonic equivalence, and the relations between equivalence and vicarious functioning.

Evidence will be drawn from previous and current research. A final section will deal with the relation of the basic "sensory-tonic" mechanisms to the more complex activity levels.

NEUROPHYSIOLOGICAL CONCEPTS BEARING ON THE "SENSORY-TONIC" THEORY OF PERCEPTION

There is a certain parallelism between the history of perceptual theory and neurophysiological conceptualization. The dichotomy between sensory and motor events was clearly expressed for the spinal level by the Bell-Magendie law. That this dichotomy extends throughout the whole nervous system was taken for granted, and reinforced by the localization experiments of Fritsch and Hitzig and their followers. Thus when the neurophysiologists took up the problem of perception as a cortical event, it was clearly in terms of this dichotomy. Generally, psychologists seem to be of the opinion that neurophysiology of today has not proceeded beyond the conception of narrowly circumscribed sensory centers of the cortex that are distinct from motor areas.

As a matter of fact the modern experimental work by Penfield and Erikson (39), as well as that of Dusser de Barenne and his colleagues, has made it necessary to revise the traditional localization concepts separating sensory and motor areas. As Dusser de Barenne, McCulloch, and others (9) have shown (using the technique of strychninization), the sensory cortex extends over a much larger part of the cortex than the traditional localization theories assumed. It occupies a large portion of the precentral as well as postcentral, parietal regions.

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[3] This relationship will be discussed later with respect to vicariousness, the dynamic exchangeability of perceptual and bodily events.

Furthermore it has been demonstrated (8) that appropriate electrical stimulation of the cerebral cortex of the chimpanzee exhibits facilitation and inhibition of the motor response throughout all parts of this wide sensory area. The main reason why previous investigators have found only a circumscribed, relatively narrow motor area (predominantly precentral) is the existence of "suppressor bands": if caution is not taken accidental stimulation of these "suppressor bands" inhibits a motor response in the postcentral area. It has also been shown that within the parietal area (traditionally called sensory area), motor response to cortical stimulation is displayed mainly if there is facilitation through stimulation of neighboring areas and support through already existing muscular tension.

These studies contradict the traditional assumption of a dichotomy between sensory and motor cortical function. They demonstrate the existence of a sensory-motor cortex, i.e., cortical regions that involve both sensory and motor functions.[4]

Experimental studies concerning the occipital lobe by Bishop and O'Leary (1) are also pertinent. These investigators analyzed the electrical activity of the occipital lobe during light stimulation of the retina. By placing electrodes at different layers of the cortex, it appeared that three groups of cells contribute to the electrical activity, namely afferent, internuncial, and cortico-fugal fibers (leading from visual cortex to superior colliculi). One might also note the experimental study by Perkins (40) on the visual cortex, which leads this investigator to the conclusion that the visual cortex has a motor as well as a sensory function.

Furthermore a great many experimental studies—the most recent ones by Fulton and his collaborators (11), by E. Hoff (17, 18), and others—on the electrical stimulation of the cortex have shown that cortical control of tonic events is not restricted to skeletal musculature, but includes autonomic activity, such as exhibited by changes of blood pressure, pulse rate, temperature, etc.

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[4] See Meyers (36) for a more general discussion.

Fulton (11) points out that:

> . . . in the cortex there is extensive overlapping between autonomic and somatic motor representation, making possible unified correlation between the reactions of the two systems. In general, the topographical relation between the cortical areas influencing specific autonomic functions is close to the cortical area influencing the corresponding somatic functions.

In a recent experimental study on the influence of the cerebral cortex on visceral activity, E. Hoff (17) concludes: "It seems quite reasonable to suppose that, in the normally functioning individual, efferent impulses which activate the somatic motor efferent will also affect the autonomic efferent mechanism."

The interrelation of somatotonic and viscerotonic cortical activity has important implications for a theory that takes into account the effect of emotions and tension on perception.

The recent evidence on cortical localization is in accord with the views of modern experimentalists and theorists on the neuropsychology and neurophysiology of motor and sensory interdependence.

Looking for psychophysiological factors most central in this interdependence, we find the striking fact that various outstanding investigators, such as Sherrington and Lashley, have emphasized the significance of tonus. On the basis of their experimental evidence in the fields of neurophysiology, neuropsychology, and psychopathology, these investigators have arrived independently at the conclusion that an intimate relation exists between motor and sensory activity mediated by what these investigators might have called tonus.

Thus the analyses of postural tonus made by Sherrington (52) and Magnus (34) have shown that the tonic activity of each spinal center is influenced by almost every sensory stimulation to which the organism is subjected, including proprioceptive, cutaneous, vestibular, visual, and auditory stimuli.

Again, Lashley (31) suggests postural-tonic factors as intermediaries between sensory and motor activities:

> The transition from the visual perceptual to the motor level appears to be the translation of one system of space coordinates into another. Direction is dominant in visual memory and the reaction reduces to a sequence of

directions of movement. Intervening between these is the system which constitutes spatial and postural orientation.

PSYCHOLOGICAL AND PSYCHOPATHOLOGICAL STUDIES BEARING ON THE "SENSORY-TONIC" THEORY OF PERCEPTION

Some of the crucial facts for a theory that stipulates interaction between sensory and tonic processes can be found in the results of a number of experimental investigations employing normal as well as abnormal individuals. One group of studies demonstrates the influence of sensory stimulation upon the distribution of tonus throughout the organism. A second group shows that tonus, either directly applied, or induced indirectly through sensory stimulation, is a formative factor in structuring the perceptual field. A third group of experiments deals with the interrelationships between the senses which suggests "sensory-tonic" interaction.

Perhaps the first experimental proof that sensory stimulation, such as light, affects tonus distribution in human beings was presented by Stein (53) and by Metzger (35). Strong illumination of one eye causes a one-sided increase of body tonus which can be measured by the tendency to fall to the illuminated side. A. Jellinek (23) applied Tullio's method of examination of sound reflexes in animals to humans, and found similarly a tonus-producing effect of auditory stimuli. Mrs. Jellinek showed further that the form of the reflex aroused by sound depended on the postural status of the organism and on the various attributes of tone. She concluded that sounds elicited tonus configurations in the organism which were specifically related to pitch, timbre, intensity, duration, and direction of sound.

Changes of this sort are very conspicuous in cases of disturbance in the cerebellum, where the motor apparatus has lost its stability. Goldstein (13, 14), H. Hoff and Schilder (19), and others have studied cerebellar patients and found that visual, auditory, and tactile stimulation asymmetrically applied affects tonus distribution as manifested by past pointing, falling to the side of stimulation, etc.[5]

Beyond the influence of sensory stimulation on tonus, there is also evidence that tonus is a significant factor in perceptual organization. The first contributor to this problem was probably Ernst Mach (33). By means of putty, Mach fixated the position of the eyes of subjects to one extreme side. When the subjects were requested to attempt to look toward the other side, they reported that objects looked displaced in that direction. Mach concluded that localization in space depended on impulses involved in the execution of eye movements.

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[5] See the studies demonstrating the effect of the visual field on posture and balance (Witkin and Wapner, **65**; Wapner and Witkin, **58**).

Kleint (25), following a suggestion by Mach, studied the perception of the vertical under varying tonus conditions. Kleint had his subjects observe a vertical line while sitting erect and counterbalancing a weight attached to the head. They reported an apparent inclination of the vertical in the direction of the increased tonus.

A number of other studies show that perceptual organization is not only affected by direct muscular changes of tonus but also by stimuli which act indirectly on the muscles. The earliest studies of this sort were probably made by Nagel (38), and by Urbantschitsch (54, 55). These workers found a displacement of the vertical by labyrinthian stimulation.

Analogously Muensterberg and Pierce (37) found displacements of auditory localization in the direction of rotation, when the subject was rotated around a vertical axis. When rotation was stopped, displacement in the opposite direction occurred. Corresponding observations were made by Hofmann (20), as well as by Dittler (7), with visual after-images and also by Kleint (25) with visual objects.

A few studies demonstrate that a sensory stimulus such as light can serve as a tonus-inducing factor and thus affect perception. Probably the best controlled investigation of this kind is that by Roelofs (42). He used a homogeneously illuminated rectangle in the dark room (see Figure 1), and asked the subject to adjust the figure so that edge A was located in the median plane. Under these cir-

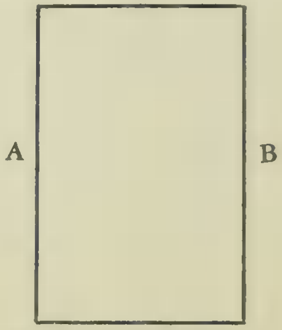

FIG. 1. Roelof's Figure.

cumstances the subject shifted the edge A to the right of the objective median plane. An analogous displacement was observed for edge B. These changes can be explained in terms of asymmetrical tonus loadings induced by light. In condition A, in consequence of the greater tonus on the right (induced by the light) the subjective median is shifted to the right, i.e., to the side of the increased tonus.

There are some observations reported which suggest that not only localization, but also size and form are influenced by tonic processes. Kleint (27) has observed that distortion of form as well as changes of size of visual objects occur when the subject is rotated around a vertical axis. It is to be admitted that this significant problem needs further investigation under well-controlled conditions. This relation between tonus and size is now being pursued in the Clark laboratory. Particularly clear evidence for the intimate relation between tonus and size comes from studies on micropsia and macropsia in patients with tonus disturbances.[6] (H. Hoff and Schilder, 19, and others.)

Related to these studies is a whole group of investigations that deal with interaction between the senses. These studies, starting with pioneer experiments by Urbantschitsch (54, 55) more than fifty years ago and carried out more recently by investigators such as Werner (60, 62), Zietz and Werner (66), Schiller (50, 51), Kravkov (30), Goldstein (15), Boernstein (2), Costa and Bertoldi (6), Gilbert (12), etc., have demonstrated a reciprocal influence across sense modalities. In the light of the evidence already mentioned on the effect of sensory stimulation on tonus, we can probably justly assume that this heteromodal interaction can be best understood in terms of "sensory-tonic" activity.

SPACE PERCEPTION AND THE EQUIVALENCE OF SENSORY AND TONIC EVENTS

If one views the history of research in space perception, one is confronted with a puzzling situation. Though throughout a whole century studies have been made about changes of the visual and auditory field depending on posture, these studies have been looked upon, for the most part, as dealing with rather marginal aspects of the problem, whereas the main emphasis has been given to the experimental analysis of the visual field as a self-contained entity. Only relatively recently have psychologists begun to realize fully the significance of experimental work in this area. By ignoring research of object-body relationships, psychologists have been blind to the deficiencies of present-day perceptual theories; any general theory is inadequate as long as it treats the perceptual field as isolated from the organism as a whole.

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[6] Note also the evidence on micropsia and macropsia as related to ocular-muscular changes (accommodation and convergence) as described by Purkinje (41), Koster (29), Jaensch (21), and others.

One might expect that the growing interest in the study of object-body relationships will not only bring about a revision of perceptual theory, but that, conversely, this area of research will become a testing ground for the validity of perceptual theories.

In appreciation of the central nature of such research, a series of experimental investigations have been initiated at the Clark laboratories that have as their principal theme the organization of perception in terms of body-object interrelationships. Guided by working hypotheses that are in accord with the "sensory-tonic" theory, the experimental program is directed toward the uncovering of basic mechanisms underlying the projective nature of perception.

We wish to report briefly some results pertaining to the effect of tonus-inducing stimuli upon perceptual organization in space, the dependence of this effect upon the existent tonus distribution of the body, and the equivalence of muscular and sensory stimulation.

In one of the dark-room situations the subjects had the task of adjusting a luminescent rod pivoted at its center to the apparent vertical; the subject was sitting erect with head fixed by means of an adjustable head rest. The first part of the experiment, where no tonus inducing stimulation of any kind was used, served as the control. Here it was observed that there was a subjective tilt of the vertical to the right ("subjective zero") as indicated by the adjustment of the rod.

In the second part of the experiment the subject made his observations while tonus was directly increased by stimulation of the motor nerve of the neck muscle through faradic current. During one set of trials the left side was stimulated, and during another set of trials the right side was stimulated. Under these conditions the subjects observed a significant angular displacement with respect to the subjective zero, i.e., the perceived vertical without stimulation.

This tilt was in the direction of the side of increased tonus, i.e., if the neck muscle of the left side was stimulated the displacement occurred to the left, and if the neck muscle of the right side was stimulated the displacement was to the right (see Figure 2 for mean values).

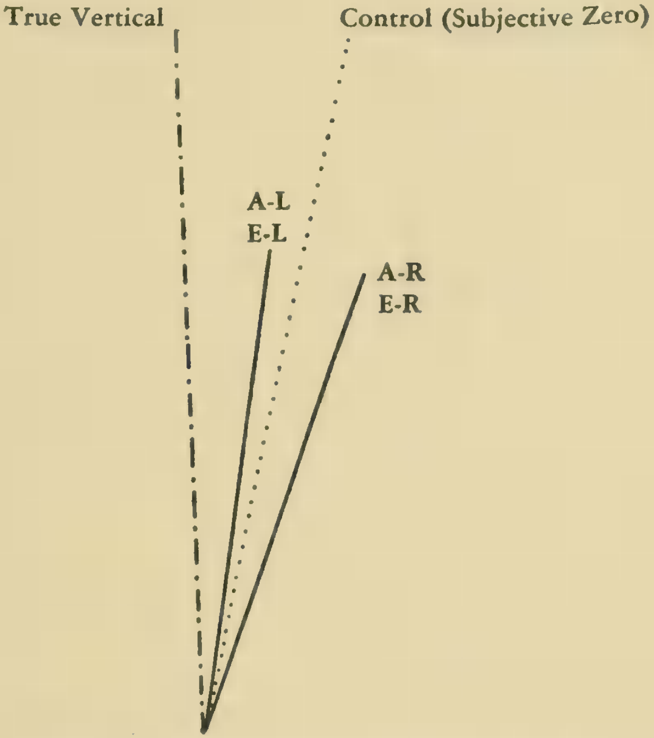

FIG. 2. Perceived vertical under direct (muscular) and indirect (auditory) tonus inducing stimulation (in the diagram the effect is magnified 10 times).

		Mean Degrees Shift
A-L:	Auditory stimulus to left side	1.0
E-L:	Electrical stimulation of muscle on left side	1.0
C:	Control (Subjective zero)	1.4
A-R:	Auditory stimulus to right side	2.1
E-R:	Electrical stimulation of muscle on right side	2.1

A third part of the experiment involved the same procedure with the exception that the subject was given one-sided auditory stimulation. An 800 cycle oscillator tone was presented by means of ear phones embedded in the head rest. Again the vertical was seen as tilted towards the side of stimulation, i.e., either to the left or right of subjective zero.

These results show that tonus—whether directly induced by stimulation to the muscle or indirectly by auditory stimulation—affects spatial perception.

Another important problem concerns the dependence of the described stimulation effects on already existing body tonus conditions. With respect to this problem one fact deserves special note, namely, that in the erect position, the true vertical is seen by most people as tilted somewhat to the right. Since this has been found in a number of our experimental studies, and by other investigators as well (**64**), it can be inferred that there is a natural asymmetrical tonus distribution which is to the right for the majority of subjects and which produces a tilt of objects in the visual field.

To pursue this problem further, experimentally controlled variations of tonus distribution in the body were introduced; this was achieved by having the subject make observations while his body was fixed in various tilted positions. A number of experimental series have been conducted that all point to the same general result: the effect of direct and indirect tonus-inducing stimuli on perception differs strikingly whether the body is erect or tilted, and furthermore whether it is tilted to the right or to the left.[7]

These results support the previously outlined theoretical statements that one cannot properly deal with perception without taking into consideration the concept of a field where interaction takes place between body and object.

The experimental findings are not only significant for the problem of "sensory-tonic" interaction but also bear on the problem of sensory and tonic equivalence. Examination of Figure 2 shows the following: Mean subjective zero (perceived tilt of a vertical line) is 1.4 degrees clockwise. Both electrical (direct) and auditory (indirect) stimulation produced a mean angular displacement of 2.1 degrees relative to the true vertical when applied to the right side; and again both stimuli produced a mean angular displacement of 1.0 degrees when applied to the left side. As will be noted, the displacement relative to subjective zero is to the right where the stimuli are applied to the right, and to the left where the stimuli are applied to the left.

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[7] A detailed report of these and related results will appear in a later publication.

This points to the equivalence of sensory (auditory) and tonic (direct muscular) stimulation; i.e., *both affect the organism in an identical way resulting in similar perceptual changes.*

The establishment of equivalence of sensory and muscular stimulation is more than an isolated result; it is crucial to any sensory-tonic theory. The fact of equivalence found in our experiments corroborates our statements presented in the beginning, that in order to account for sensory and tonic interaction logically one needs the construct of a "sensory-tonic" field which becomes established through common dynamic properties of sensory and bodily processes.

EQUIVALENCE AND VICARIOUSNESS

The dynamic equivalence of tonic and sensory factors is expressed in still another way, namely in terms of vicariousness of function. By vicariousness we mean that equivalent functions may serve as substitutes for each other with respect to an end product. This end product may be the bodily activity, the perceptual object, or the object-body relationship in the total field. Accordingly we should like to refer to three areas where vicariousness has been established, namely (1) with respect to bodily activity (motor-tonic vicariousness); (2) with respect to the perceptual object (sensory vicariousness); and (3) with respect to relationship between object perception and body activity (sensory-tonic vicariousness).

Illustrations of vicariousness with respect to bodily activity can be found in certain types of motor pathology, such as athetosis and chorea. Overactivity due to lesions in the extrapyramidal sector leads, as it is known, to involuntary movements in restricted bodily areas. If this motion is prevented by surgery or some other method, overflow of activity will appear in other parts of the body.[8] Thus vicariousness with respect to bodily activity expresses itself by the exchange of areas in which the movement occurs.

Again, with respect to the perceptual object, a number of facts are know which point to equivalence of so-called "attributes of sensation" in the visual, tactual, or auditory field. One well-known example is the interchangeability of variations of intensity, size, and distance in the perception of apparent motion (Korte's laws). Another illustration is the translation of size changes into three-dimensional space as discussed by Koffka (28), or, for that matter, the various sensory cues which may substitute for one another in depth perception.

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[8] See Schilder (**48**, **49**), e.g., for cerebellar cases, and the literature on corrective surgery in athetosis.

Vicariousness of function is, however, confined neither to the bodily activity nor to the perceptual object alone. New experimental evidence is appearing which points to vicariousness with respect to object-body relationships. This is seen clearly if we study perception under the condition where the body is included as an intrinsic part of the experimental situation. As an illustration we might refer to Vogel's (56) standard situation where the subject stands in the center of a striped cylinder rotating around him. Two forms of behavior are observed. In the first, the screen is seen as moving, the observer follows the striped screen by oculomotor activity (nystagmus), and frequently this activity involves the whole body, i.e., the observer turns in the direction of movement. In the second, the screen is seen at rest, the motor activity—nystagmus, as well as motion of the body—has subsided and the observer now perceives apparent motion of a fixation point in front of the striped screen, together with an apparent motion of his own body in the opposite direction. Vogel analogizes the happenings in these two situations by comparing them to the reversal that occurs in mirror images: with the perception of illusory motion, physical movements in the opposite direction cease; and with the emergence of bodily motion, illusory motion disappears.

Similar observations concerning the inverse relationship between perception of apparent motion and bodily activity were made by Roelofs and Walls (43) with normals, and in psychopathological cases by H. Hoff and Schilder (19), and by Goldstein (13).[9]

In the face of this evidence we may postulate that the available "sensory-tonic" energy may either be released through body movement or may express itself in perceptual displacement or apparent motion. Weizsaecker (59) has called these reciprocal processes extraversion (body movement) and introversion (perception of movement). As Werner (61) pointed out, this evidence may have a bearing on personality as reflected in projective perception, and is obviously in accord with Rorschach's (44) interpretation on the relation between introtensive personality traits and the perception of movement in ink blots.

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[9] For further evidence, see Werner (**61**). Analogous results are obtained in situations where the subject is rotated and the screen remains stationary (Kleint, **26**). See also Wallach's (**57**) experiments on auditory localization.

We may add that further evidence pointing to "sensory-tonic" equivalence and vicariousness with respect to object-body relationships might eventually lead to a more general view of psychobiological dynamics that utilizes energy concepts. The possibility remains, then, that a concept such as energy transformation, which appears as a leading construct in psychoanalysis, may emerge and be shaped in such a way that its validity can be tested by rigid experimental procedure.

Levels of Complexity and "Projective" Perception

In terms of the "sensory-tonic" field theory—which takes cognizance of organismic factors—so-called "projective" perception is but a particular instance of perception in general. It is true that compared with "projective" perception, as reflecting the more complex motivational structure of personality, the object-body interdependence revealed in our experiments operates on a level of great simplicity. In other words, if one studies the effect of tension or "tension release" upon size (5), or the relation between emotion and color response, one is dealing with a much more complex level in which conative and cognitive factors are inextricably related. Though the level of complexity may vary, the basic mechanisms involved in all perception, whether "projective" or not, is the same.

The view that levels of complexity exist leads to the further problem of the relation between these levels. The concept of the organism as a whole should not be given lip service but should be taken seriously. One might ask: In what way are happenings on the basic levels related to happenings on the more complex levels?

Here one might take advantage of some of Schilder's (48) hypothetical constructs in the area of psychopathology for the understanding of normal personality. Schilder makes use of the concept of hierarchical organization of the person in his interpretation of psychopathological cases where there are signs of disturbance or primitivation of motor and tonic functions (extrapyramidal and autonomic system involvement). In cases of this sort, such as hysteria and schizophrenia, there can be postulated a linkage between the symbolic, "projective" level and a primary level of skeletotonic and viscerotonic activities. For instance, Schilder presents the results of an analysis of a schizophrenic who primarily displayed an increased activity of the postural and righting reflexes together with delusions that he could change the universe by his movements. Schilder interprets the patient's idea of being able to turn the world upside down as a formulation or utilization of the pathological changes that take place on the primary level of body activity, consisting in the lability of the righting reflexes. Another case is that of a patient with a tic, psychogenic in origin. The tic consisted in the turning away of the head. Schilder suggests an organic disturbance within the extrapyramidal system as the primary level of the symptom, whereas at the symbolic level the tic is signified as a withdrawal or turning-away response.

Schilder's formulations give some suggestions as to how one might conceive of the relation between the basic perceptual mechanisms as postulated here and the level of symbolic or "projective" perception. It might not be too bold to assume that attitudes and motivations affect the "sensory-tonic" (particularly viscerotonic) state of the organism, and it is through these basic mechanisms that attitudes or motivations may "project" themselves into perceptual objects. Thus somatotonic and viscerotonic events may be specifically expressed, projected, or channelized at a higher level in accord with the specific—transient or enduring—need structure of the individual.

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INTOLERANCE OF AMBIGUITY AS AN EMOTIONAL AND PERCEPTUAL PERSONALITY VARIABLE

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INTRODUCTION

In recent years there have been an increasing number of attempts to integrate the field of perception with that of motivation and of personality. These attempts may take either of two opposite directions. One is characterized by a basic dependence on the outlook, the problems, and the techniques developed in the general psychology of perception. This approach may be further subdivided in the following manner. The motivational angle is either, first, superimposed upon this nucleus of general problems of perception as an additional element or factor, or second, derived from it by an expansion of concepts or principles originating in the field of perception. Both these subdivisions may be characterized as perception-centered.

The opposite direction is taken by a third group of investigations. These are organized about major patterns of findings originating in some area of personality research; problems or findings of perception-psychology are brought into the picture by way of analogy as convenient means in the testing of hypotheses, or in some other secondary role. This may be called the personality-centered approach.

This is not to say that with the first two cases the primary interest is always in perception, and with the third always in personality. On the contrary, the decisive slant may in each case be given by the new element of thought injected rather than by the core to which this new element is added.

In the present paper the third, personality-centered, approach is applied to concepts and findings originating in the sphere of emotional ambivalence and to their expansion into experiments on perceptual ambiguity proper and on related subjects which had been treated under different aspects so long as they were left within the framework of the psychology of perception.

PERCEPTION-CENTERED STUDIES IN MOTIVATION AND PERSONALITY

Examples of the first subvariety of the perception-dependent approach, that in which motivation is superimposed upon a problem rooted in the tradition of the general psychology of cognition, are given by Murray's investigation of the influence of fear on perception (25), Sherif's work on the need for conformity (32), or Bruner and Goodman's experiment on values as determining perception in the poor as contrasted with the well-to-do (3). Emphasis is in each case on temporary or otherwise relatively personality-alien factors, defining a single motivational force in relative isolation. Characteristically, the experimental task itself is one borrowed from the inventory of routine perception problems. It may involve the traditional problem of geometrical "illusion," or it may be an aesthetic appreciation problem previously limited to a more formal approach. Want, fear, or social influences are now added as further determinants to complete the existing list of interferences already known. And while the motivational factor is superimposed upon a well-structured cognitive core, it remains unrelated to the basic personality of the subjects, at least within the framework of the experiment concerned. The relative conceptual isolation in which the new factor is usually presented has led to the accusation that in this kind of work the traditional cleavage between cognition and emotion is needlessly continued. Use of such expressions as "influence of needs or values upon perception" has given further nourishment to this interpretation. Murphy (24) has characterized the point of view discussed by the phrase "needs keep ahead of percepts." /

Examples of the second subvariety of the perception-dependent approach is given by a group of investigators who have come to conceive of personality as a whole after the pattern of what they have learned in studying perception proper. This role may well have fallen to the Gestalt psychologists had they been able to develop a sufficiently specified interest in problems of personality.[1]

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[1] Among the more recent expansions of the psychology of perception, the farthest advance into a special field of application under the Gestalt point of view has been made by the social psychology of Krech and Crutchfield (20).

The challenge was, for some time, left in the hands of the characterologists. Among the perceptually loaded variables that have become prominent in this course of development is the one defined by the opposition of the *synthetic* and the *analytic* approach as adopted, among others, by Kretschmer and his school (notably in some later studies summarized in an as yet untranslated new chapter on experimental type-psychology in the 1936 and 1942 editions of his *Körperbau and Charakter,* **22**) and also represented by the *whole* vs. *detail* emphasis in the evaluation of the Rorschach Test (**30**); further pairs of opposites borrowed from the psychology of perception are *diffuse* vs. *articulated* (Werner, **35**), *concrete* vs. *abstract* (Goldstein and Scheerer, **16**), as well as such rather specific distinctions as *color-dominance* vs. *form-dominance* (Kretschmer, Schroll, and others, and again Rorschach; see also the treatment in Thurstone's factorial study of perception, **33**).

A case by itself within this second group is E. R. Jaensch and his school (**17**, and elsewhere). In his later years Jaensch has gone considerably beyond good evidence in postulating a "unity of style" within each individual so that a few simple perception tests would suffice for universal diagnosis. Although his tests are undoubtedly of greater potential relevance than most of the older psychophysical tests, none of them reaches the level of penetration of what has recently become known as projective techniques. Examples of the tests used by Jaensch are the adjustment of the individual to prismatic lenses which at first will make straight lines appear curved, the persistence of movement phenomena after viewing a rotating spiral, exaggerated reactions to binocular disparity, and other visual illusions, along with investigation of vivid, "eidetic" images. Erratic responses are taken as a sign of general instability; there is a violent rejection of ambiguity in reactions of any form. The latter is taken as an expression of a pervasive "liberalism" which Jaensch considers morbid in the perceptual as much as he does in the political sphere.

It is primarily the synesthetic or S-type that is said to display such liberalism. This is the type whose "spatial perceptions are unstable, loosened up, even dissolved. Normally the objects of the external world are given to the psychophysical organism of man in a univocally determined spatial order. To the points of the retina correspond firmly and univocally determined locations in visual space or, as this is usually expressed, the spatial values of the retina are fixed." (Note the gross incorrectness of this statement.) "This coordination between stimulus configuration and perceptual Gestalt is disrupted in the case of the S-type" (**17**, p. 37). Liberalism of every kind—and adaptability in general, which is being summarily depreciated—goes with a lack of strong ties, according to Jaensch: "The lytic S-type has no firm tie with reality. In fact he has no ties at all. He is the liberalist at large" (**17**, p. 44). "This social liberalism is paralleled by innumerable other forms of liberalism, all of them mentally rooted in the S-type: liberalism of knowledge, of perception, of art, etc."

By contrast, the firmness, consistency, and regularity of the desirable "integrated" or J-type is being stressed over and over again. These characteristics, according to Jaensch, can be seen in the "elementary realm" of sensory phenomena as well as in the most complex cognitive and social attitudes. The J-type is said to accomplish his adaptation to the prismatic spectacles slowly and consistently, showing steady progress, whereas its undesirable counterpart, the S-type, is said to show in all the perceptual experiments irregularities, great latitude of reaction, instability, and jumpiness; his adaptation to the prismatic spectacles is said to proceed in a jerky fashion, with switching back and forth and a wide range of correction values.

Jaensch claims that the same tendencies exist in memory, imagery, and thinking. Thus, the free associations of the J-type are said to connect what belongs together, whereas those of the S-type connect what is separate, ignoring realistic relationships. The drawings of the J-type are said to be reality-near, those of the S-type reality-removed. In an experiment on the recall of pictures, the J-type is said to be more faithful and to like specific questions, whereas the S-type is said to recall a greater quantity and to give a report that is freer and of lesser fidelity, once more revealing, according to Jaensch, a morbid lack of ties to sensory reality. A rigid, rational superstructure and adherence to rational methods is said to be used by this type as a superficial safeguard against his instability on the more elementary biological levels.

The sampling technique and the statistical significance and validity of the works of Jaensch is of the shoddiest kind and cannot stand up under scrutiny even of the mildest kind. As a mere theory, the system of Jaensch is of interest to us for two reasons. First, his typology centers about concepts of ambiguity. Second, he postulates a connection between various areas of ambiguity in what he dogmatically considers the unitary "style" of liberalism, perceptual, cognitive, and social. As will be shown in this paper, all three of these liberalisms must be considered definite assets rather than liabilities, quite contrary to the opinion of Jaensch. But he was probably right in seeing a connection between these apparently widely separated fields, although he himself has not gathered or intercorrelated material on all the aspects necessary to make such a statement, and although he has a most distorted valuation of the trait of liberalism. (For a criticism of the juxtaposition of machine-like precision and excessive subjectivity see below.)

FROM EMOTIONAL AMBIVALENCE TO COGNITIVE AMBIGUITY. PERSONALITY-CENTERING IN THE PRESENT APPROACH

The approach used in the present paper differs from those outlined so far in that the problems to be discussed are originally developed and perfected in the clinical and social fields; they are reformulated and broadened here to absorb certain fitting elements of recent perceptual thought. It is this that we mean when we say that our approach is personality-centered rather than perception-centered.

The prime concern is to bring together a variety of aspects in order to study the generality or lack of generality of the personality patterns involved, that is, the readiness to spread from one area of manifestation to another. Can basic formal attitudes such as subjectivity, rigidity, fear of ambivalence and of ambiguity, etc., be taken as unified traits of the organism, or are we to find a more differential distribution, varying from one area to another?

A second advantage offered by the carrying of personality problems into perception is that tendencies which in the social and emotional fields are manifested in a vague fashion are rendered more clearly accessible to experimental verification.

It is hoped that by discussion of the same formal principles in all three, the motivational, social, and perceptual contexts, greater conceptual clarity can be achieved than through a discussion of any of those channels alone. To the richness of motivational concepts clarity will be added by nailing them down on the more objective and precise perceptual level. As will be seen, differentiations lost or obscured in the globality of the motivational approach can be re-established in this manner; on the other hand, problems in perception will gain in significance and fruitfulness if questions in personality and social psychology are allowed to help in the choice of problems. The gain is far from being restricted to the conceptual level. Important empirical relations are revealed along with clarification, throwing further light on the first-mentioned problem of the generality vs. specificity of behavior as well as on problems concerning the interrelation of the levels of personality.

A third advantage is what may be called the reduction of social bias. By the shift of emphasis from the emotional to the perceptual area, certain preconceived notions which only too readily slip into the investigation of social and clinical issues can be greatly reduced and controversial issues delineated and at least indirectly decided on a more nearly neutral platform.

The last-named two advantages are, of course, contingent upon the first point in that any shifting of problems to other areas is valid only to the extent to which the mechanisms involved throughout these areas or levels tend to be similar within the same individuals. Only a beginning will be made in this paper toward answering this problem in an empirical manner. The major emphasis will lie on the following two points: (1) on a development of the underlying reasoning from previous evidence based on the writer's own work as well as on sources from the literature; and (2) on a showing of possible ways of attacking the problem experimentally. Experimental evidence as cited below, although scattered or preliminary and by no means conclusive thus far, partly points in the direction of the generality of the personality traits mentioned, partly is suggestive of compensatory relations which may make for the simultaneous presence of opposite tendencies.

The topic to serve as a medium through which this procedure will be followed is given by what seems to this writer one of the basic variables in both the emotional and the cognitive orientation of a person toward life and what she has suggested be labeled "tolerance vs. intolerance of ambiguity" (10, 11, 12).

The material evidence is based primarily on a project conducted at the Institute of Child Welfare of the University of California[2] and dealing with rigid adherence vs. disinclination to ethnic prejudice in children, and the motivational and cognitive correlates of these social attitudes. This study has involved, to date, the construction of a series of scales for the direct and indirect measurement of prejudice[3] and for the assessment of factors suspected to be related. Data have been collected from 1500 public school children, eleven to sixteen years old, in several samples. With 120 of these subjects, representing the extremes of prejudice and of freedom from prejudice, more intensive studies were conducted in terms of individual interviews and projective tests. Furthermore, the parents of the children interviewed were visited and likewise interviewed. Approximately forty of the intensively studied children were further submitted to some of the experiments in perception proper and in memory which are to be described below.[4] An advance report of some aspects of this project has been given by the present writer not long ago (**11**, **12**). Those of her collaborators who worked on the aspects discussed here will be mentioned in the proper context later in this paper.

The line of argument will now be outlined in somewhat greater detail. Starting from the observation that some of her subjects were able to tolerate emotional ambiguities better than others, the writer became involved in the question of whether this attitude of intolerance of more complex, conflicting, or otherwise open structures extends beyond the emotional and social areas to further include perceptual and cognitive aspects proper.

The importance of individual differences in the insistence on unqualified assertions was first brought to the attention of the writer in a study on mechanisms of self-deception conducted at the University of Vienna before the war (**7**). The greater the definiteness and lack of shading, that is, the greater the intolerance of ambiguity, in the self-description of favorable traits, the less were such assertions as a rule verified in the judgment of close acquaintances.

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[2] The writer is indebted to Dr. Harold E. Jones, the director of the Institute, for giving her the opportunity to carry out this project as well as for suggestions regarding its execution.

[3] This was done in collaboration with Murray E. Jarvik and Milton Rokeach. A joint publication is in preparation.

[4] While the project on social discrimination in children referred to here has some features in common with the project on adults referred to under (**1**), it is distinguished from the latter, among other things, by the use of experiments and a greater emphasis on cognitive factors in general.

The background of the problem of ambiguity thus becomes related to the vast fund of knowledge supplied by psychoanalysis in connection with the development of the concept of "ambivalence," as defined by the coexistence, in the same individual, of love- and of hate-cathexis toward the same object. The existence of ambivalence in a person and the further fact of this person's ability to face his or her ambivalences toward others must be considered an important personality variable. As in other areas of personality, psychoanalytic statements referring to content, such as attitude toward parents, repression of certain id-tendencies, etc., are perhaps difficult of access to experimental verification. Here, however, we are dealing with experimentation concerning formal factors, such as intolerance of ambiguity, or rigidity, and the question of whether or not these attitudinal variables are restricted to the emotional area.

From here, a first step toward a more cognitively slanted reformulation is achieved by shifting to the problem of the recognition, by one and the same individual, of any actual coexistence of positive and negative features in the same object, e.g., in parents or other "in-groups." Ability to recognize such coexistences in all likelihood constitutes another important, emotional-cognitive, personality variable, which is not to be confused with emotional ambivalence in the original sense of the word. At the end of a scale defined by this ability stand those with a tendency to resort to black-white solutions, to arrive at premature closure as to valuative aspects, often at the neglect of reality, and to seek for unqualified and unambiguous over-all acceptance and rejection of other people. The maintenance of such solutions requires the shutting out of aspects of reality which represent a possible threat to these solutions. It is this problem of "reality-adequacy" vs. "reality-inadequacy" which injects a distinctly cognitive element into the broader sphere of the problem of ambivalence.

Our material gives evidence of individual differences both in emotional ambivalence and the readiness to face it, and in the more cognitive recognition of traits of conflicting value in others. Since the clinical aspects of the problems involved have been discussed in greater detail elsewhere (**11, 10, 1**), they will be presented here in a summary fashion only.

Some individuals are more apt to see positive as well as negative features in their parents and can accept feelings of love and hate toward the same persons without too much anxiety or conflict. Others seem compelled to dramatize their image of the parents in seeing them either as altogether good or as altogether bad. The following question suggests itself in this context: Is this second attitude an intolerance of an existing underlying ambivalence, or is it merely absence of ambivalence? Why do we doubt that a thoroughly positive description of the parents with denial of any negative features or discrepancies as given to us by some of the children is not a true representation of the children's feelings? On what basis are we entitled to claim that the individual has repressed the negative side of an ambivalence out of some hypothetical intolerance of ambiguities and complexities?

We list here only a few of the facts which were used as the basis of such an inference. First, the description of the parents in the second type of cases is often stereotypical and exaggerated, indicating a use of clichés rather than an expression of genuine feelings. The range of responses in such cases is rather narrow and without the variations commonly found in the description of real people. Only the more palpable, crude, and concrete aspects are being mentioned. Thus we find a preponderance of references to physical and other external characteristics rather than mention of more essential and abstract aspects of the parents' personalities. Futhermore, a child relating only positive feelings while talking directly about his or her parents may at the same time reveal a negative attitude on manifestations which are more indirect. For instance, he may omit his parents from the list of people he wants to take to a desert island. Or, when describing parents in general rather than his own, e.g., in his responses to parental figures on the Thematic Apperception Test, he may stress the coercive and punitive aspects of parents. It is data of this and other kinds which induce us to state that the children concerned split the positive and negative side of their feeling and attitudes rather than become aware of their coexistence.

Both the exaggerated concreteness and the stereotypy of the descriptions can serve as diagnostic indices of dynamic states. The above considerations concerning the diagnosis of the "real" emotional state are a further demonstration of the greater conceptual power of the depth-psychological approach. The latter rests upon the inclusion of minimal cues as well as on material of an indirect type such as projective techniques. For a more detailed discussion of the relationship of the underlying genotypical or conceptually more derived level to the manifest or phenotypical level of personality as relevant in this context, see Frenkel-Brunswik (8, 9).

Synopsis of a variety of data suggests that the attempt to master aggression toward parental figures who are experienced as too threatening and powerful are among the important determinants of the tendency rigidly to avoid ambiguity of any sort.[5] The requested submission and obedience to parental authority is only one of the many external, rigid, and superficial rules which such a child learns. Dominance-submission, cleanliness-dirtiness, badness-goodness, virtue-vice, masculinity-femininity are some of the other dichotomies customarily upheld in the homes of such children. The absoluteness of each of these differences is considered natural and eternal, excluding any possibility of individuals trespassing from the one side to the other. There is rigid adherence to these clearly delineated norms even if this implies restrictions and disadvantages for the own group. Thus, not only boys but also girls exhibiting the need for dichotomizing subscribe to restrictions for women rather than expose themselves to more flexible but at the same time more uncertain norms.

In line with this, in the type of home just referred to, discipline is experienced by the children significantly more often as threatening, traumatic, overwhelming, and unintelligible, as contrasted with an intelligent, non-ego-destructive type of discipline in the home with the more flexible atmosphere. Actually, in the home with rigid[6] orientation the discipline is more often based upon the expectation of a quick learning of external, rigid, superficial rules beyond the comprehension of the child.[7] Family relationships are based on roles clearly defined in terms of dominance and submission. Some of the children live in a situation comparable to permanent physical danger which leaves no time for finer discriminations and for attempts to get a fuller understanding of the factors involved but in which quick action leading to tangible and concrete results is the only appropriate behavior.[8] It is of course true that no child can fully master his environment. Global, diffuse, concrete, undifferentiated types of reaction have thus been described by Werner (35) and others as characteristic of the child in general. It depends on the atmosphere of the home and the more specific expectations regarding the child's behavior, however, whether such reactions become fixated or whether progress toward higher developmental stages is being encouraged. For the latter course a reduction of fear and a tolerance toward weakness in the child are necessary.

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[5] For a discussion of attitudes toward authority and the resulting patterns of life, see also Fromm (14).

[6] Few concepts in recent personality psychology have been used with such a variety of connotations as that of rigidity. Unless otherwise specified, the term "rigid" refers in this paper to the various kinds of intolerance of ambiguity discussed.

[7] Most recently, the relationship between frustration and rigidity was experi-

Further factors contributing to the rigidification of personality in children are the stress on stereotyped behavior, an expectancy of self-negating submission, and the inducement to repress nonacceptable tendencies. As a result we find a break and conflict between the different layers of personality which contrast sharply with the greater fluidity of transition and intercommunication between the different personality strata of the child in the permissive home. Repression and externalization of instinctual tendencies reduce their manageability and the possibility of their control by the individual, since it is now the external world to which the feared qualities of the unconscious are being ascribed.

Data on the parents of the children in the rigid, intolerant group reveal that it is their feeling of social and economic marginality in relation to the group to which they aspire from which ensues the desperate clinging to external and rigid rules. These parents report significantly more often their own parents as foreign born, indicating perhaps that they still see themselves entangled in the process of assimilation. They well may be furthering everything which they deem advantageous and repressing everything which they deem

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mentally demonstrated by Christie (**5a**). In a task similar to that by Rokeach as described below (see also **29**) subjects exposed to a frustrating situation given by an unsolvable reasoning problem persisted, more often than a nonfrustrated control group, in a set maladaptive to the task.

[8] For the relationship between frustration and rigidity, see especially Rosenzweig (**31**).

detrimental to this goal. Obviously, the less secure they are in their feeling of belonging the more they will insist on maintenance of the cultural norms in themselves and in their children. It is this rigid adherence to norm which furnishes the key to an understanding of all the various avoidances of ambiguities listed in this paper.

In order to reduce conflict and anxiety and to maintain stereotyped patterns, certain aspects of experience have to be kept out of awareness. Assumptions once made, no matter how faulty and out of keeping with reality because of a neglect of relevent aspects, are repeated over and over again and not corrected in the face of new evidence.

The spontaneous interview statements of children in the rigidly intolerant group show a tendency toward polarization similar to the one referred to above. Often this is done in moralistic terms avoiding any qualification or expression of conflicting feelings. The evidence from both direct and indirect material thus suggests that children who tend to make unambiguous statements, either of total acceptance or of total rejection, seem to be aware of only one of two aspects coexisting within their dynamic, attitudinal make-up. A static superstructure appears to be superimposed upon a most conflict-ridden understructure, with resulting major discrepancies and stresses between the two levels. In this state of affairs the conflicting tendencies are isolated from each other and expressed only alternatively through different types of media, each of them representing a different layer of the total personality.

The fact that the tendency toward emotional dichotomizing in inter-personal relationships is related to dichotomizing in the social field is not a surprising one. In analyzing, say, the aspects contained in racial prejudice, one is first reminded of the more general dichotomy of ingroup vs. outgroup in its more rigid form that precludes the possibility of crosspassing. A further feature is that the criterion for the distinction is usually an external one, such as color or place in the social hierarchy. Moreover, the distinction tends to become a totalitarian one in that all of the "good" characteristics are ascribed to those of the ingroup and all of the "bad" ones to the outgroup. There is, finally, the introduction of a double standard of values as a further type of dichotomy. (For an analysis of the various dimensions of ethnocentrism, see **23, 1.**)

In order to maintain the rigid and moralistically tainted distinctions just mentioned, one and the same trait—e.g., erudition or aggressive orientation toward success—is considered objectionable in the outgroup yet definitely desirable in the ingroup. There is little awareness of the ambivalent or ambiguous quality which these traits acquire by this double-assessment. Both ingroup and outgroup are thus characterized by a Gestalt which has great "pregnance" (is highly clear-cut), as was noted above for such concepts as that of masculinity and femininity. These Gestalten are "closed" and cannot be modified by new experiences, which are immediately viewed from the standpoint of the old set and classified in the same way as the previous ones. Such tendencies as premature closure, jumping to generalizations on the basis of certain specific and external aspects, carrying over old sets, and the like, will become evident in the cognitive and perceptual reactions as they are evident in the emotional and social spheres. "Speed and strength of closure" turned out to be one of the major factors in Thurstone's analysis of perception (33). The question which is up for discussion in this paper is whether or not to see *Gestalten* which are too "good" in the sense of the principle of *Prägnanz* is an even more generalized type of reaction, although the seeing of relatively circumscribed and closed *Gestalten* may be at the price of not being able to perceive the broader over-all connections.

One must add that a great variety of perceptual and cognitive processes may be considered as formal approaches to reality which may be very revealing of a person's style of life. The establishment of the relationship of these formal aspects to content is an important task of psychology. The question may be raised as to what are the consistencies, and positive correlations, and what are the compensations in terms of behavior. It is such problems which led to the experiments discussed subsequently in this paper.

Concentrating on tolerance of ambiguity, the question takes the form as to whether those incapable of conflicting emotions—or of conflicting value judgments—are generally incapable of seeing things in two or more different ways. It is at this point that a well worked-over area of psychological research is again being approached. This is the area covered by the concept of perceptual ambiguity *(Gestaltmehrdeutigkeit)* as originally defined by Benussi (2) and as further developed in the well-known work of Rubin on figure-ground reversals and in the studies of the Gestalt psychologists proper (see Koffka, 19). In its own right, this problem of cognitive ambiguity is of very broad scope. In its more recent ramifications it includes the problems posed by the fact that ambiguity of cognitive responses must be seen as a reflection of the uncertainties existing in the environment itself, thus opening up the field of "probability" as a new area of psychological research (see Egon Brunswik, 5).

Academic research on ambiguity and on probability adjustment has discovered a number of important principles by which cognitive responses are linked to characteristics of stimulus-configurations and stimulus-combinations in the environment, such as to the relative size of the figure vs. ground area, or the relative frequency of reward on the two sides of a rat maze, respectively. In turn, problems of personality patterns with respect to ambiguity such as those outlined in some of the preceding paragraphs were as a rule by-passed; in the typical case, individual differences in ambiguity reactions are rarely mentioned.

In the type of study with which the present paper concerns itself, this procedure is reversed. There is no more than a passing interest in the external conditions of ambiguity, basic in perception psychology. The entire development of the problem is under the aspect of personality. Issuing from this latter aspect, excursions are made into perception psychology, adapting some stock experiments or taking them as cues for new experiments but always subordinating them to patterns of personality differences as the basic problem, with an eye to developing them eventually as diagnostic tools or tests.

This personality-centered rather than perception-centered approach may be linked to certain further developments within psychoanalysis proper. An example of the occasional parallels between motivational and perceptual processes drawn in the psychoanalytic literature which at the same time is related to our own problem can be found in Fenichel's discussion of the compulsive character (6). After an examination of the general dynamics of this syndrome Fenichel describes the need for being systematic and for clinging to definite systems as it occurs in the compulsive. This need, often manifested in the tendency to "type" and to classify in categories, is seen as protection against surprise and fear of drive impulses.

Deviations from symmetry are not tolerated but are experienced as deviations from general norms, especially moral. Compare this depth-psychological explanation of the tendency toward symmetry with the one given by Gestalt psychology in terms of dynamic factors in the brain field and you have a further illustration between personality-centered and perception-centered approach.

The clinically well-known mechanism of isolation found in the compulsive character can, according to Fenichel, lead to an inhibition to see "Gestalten" and to a perception of a sum of elements instead. Related to this is said to be the preoccupation with small, insignificant detail which often is found to be taken as a symbol for more important aspects of objects. In psychoanalysis, assumptions of this kind are as a rule not based upon actual experiments in perception or cognition. In some cases such experiments can be easily undertaken, however. In fact, the experiments referred to below bear decidedly upon the topics just described.

In order to investigate empirically how far basic personality trends, found in the emotional and social sphere, such as ambivalence, are apt to spread beyond this area to include perceptual and cognitive aspects, it was decided to combine the personality studies of the children in our project on ethnic prejudice referred to above not only with an ascertainment of their social beliefs but also with an investigation of their perceptual reactions. To quote from the theoretical considerations in an advance report of 1945, the writer had been led "to expect prejudice to be associated with perceptual rigidity, inability to change set, and tendencies to primitive and rigid structuring of ambiguous perceptual fields. Well-tested experimental approaches are available for these variables. It only remains to use them in connection with susceptibility to prejudice"[9] (see also **11**).

At first the problem was approached in a more summary fashion. Such traits as "intolerance of ambiguity," "distortion of reality," and "rigidity" were defined in a general manner, and each child was rated, without knowledge of his prejudice score, on the basis of a synopsis of the available clinical material. The same group which manifested extreme racial prejudice received on the average high ratings on these traits also.

[9] The report from which this passage is quoted has been written for the Research Department of the American Jewish Committee which sponsored the first two years of the project on social discrimination in children mentioned above.

Furthermore, children with a tendency to dichotomize in the social field on the basis of external characteristics—i.e., the ethnically prejudiced children—at the same time tend to subscribe to statements included in a personality inventory and expressly designed to reveal a dichotomizing attitude, a rejection of the different, or an avoidance of ambiguities in general. Examples of the statements used are:

People can be divided into two distinct classes: the weak and the strong.

Teachers should tell children what to do and not try to find out what the children want.

Only people who are like myself have a right to be happy.

Girls should learn only things that are useful around the house.

Refugees should be thrown out of this country so that their jobs can be given to veterans.

There is only one right way to do anything.

To this more general evidence of the relationship between emotional-social ambivalence and its repression, on the one hand, and tolerance of cognitive ambiguity, on the other, we now add a brief description of some relevant experiments, some of them still in a highly tentative state.

DIFFERENTIAL DISTORTION OF REALITY IN AN EXPERIMENT ON MEMORY

First a memory experiment will be discussed in which there is still some emotional and social involvement. Among other things, it will bring out closeness of opposites, a point which has been stressed above as an important characteristic of the personality-centered approach.

The task was the recall of a story. It was carried out in 1946 as a group experiment with forty-two children of the 6th, 7th, and 8th grades to whom a number of tests developed in our above mentioned project had been given. In this case, no selection of subjects was made in terms of their standing on the prejudice scale, so that the sample includes extremes as well as subjects of intermediate attitude. The writer is indebted to Mr. Murray E. Jarvik and Mr. Donald T. Campbell for their participation in the construction of the experiment, and to Mr. Leonard Gordon for his assistance in evaluating the material.

The "story"—actually a somewhat broadened milieu characterization—deals with the pupils of a school and their attitudes toward newcomers. In an introductory paragraph one short sentence each is devoted to examples of the boys in the school, labeling them by first names and giving one or two facts about each of them relating either to their individual habits or achievements—such as playing the violin or having been on a radio quiz—or to their fathers' occupation, economic status, religious affiliation or ethnic and racial group membership. Of the eleven children thus introduced one is a Negro and one is Jewish. The major part of the story starts out by listing three of the boys as newcomers to the school. It then proceeds to give short generalized sketches of the behavior of a number of the old-timers toward the newcomers in terms of aggressiveness vs. protectiveness in the fighting that develops against the newcomers. The story ends with a description of the somewhat futile efforts of the newcomers to defend themselves. The entire material covers one and one-half double-spaced typewritten pages. With its many participants it is deliberately somewhat confusing. To render accuracy even more difficult, it was read to the children only once. After a short interval they were asked to reproduce it in writing.

In the context of the present paper we may conceive of the story as a piece of reality and ask ourselves what changes this reality undergoes in the memory of the children, especially also in the direction of an elimination of ambiguities and other complexities.

As was to be expected, children scoring relatively high on prejudice mentioned the Negro boy significantly more often in an unfavorable context, with or without explicitly referring to his being a Negro, than did the less prejudiced. The negative characteristics ascribed to him were subjective elaborations on the part of the subjects concerned since the story itself says no more about him than that his "father was a Negro and worked in a hotel."

The negativistic tendency in the distortion of story content on the part of the prejudiced children is not limited to the description of this particular boy with his minority status. The prejudiced children tend generally to recall a higher ratio of undesirable over desirable characteristics. This result is in line with the general overemphasis on negative, hostile, and catastrophic features found in the clinical data, the interviews, and the Thematic Apperception Test stories of the highly ethnocentric subjects.

It is further to be noted that in those scoring low on ethnic prejudice, the ratio of undesirable to desirable features recalled is closer to the ratio in the story itself. In short, low-scorers stuck closer to the "truth," in this respect at least, than did high-scorers. Over and beyond the hostility mentioned above the distortions of the high-scorers tend not only to be more frequent but also to be of a cruder nature. All this is revealed by the fact that 43 per cent of the high-scoring children, as contrasted with only 8 per cent of the low-scorers, recalled exclusively that part of the story which deals with the fighting without mentioning any of the other themes (statistical significance is at the 1 per cent level of confidence). It may be added that evidence from the interviews likewise points toward the relatively great attraction which fighting has for this group of children. In the recall of the prejudiced children the story gets generally more simplified and less diverse. Often a unified attitude of aggressiveness is assumed toward the newcomer, with the only theme, fighting, sharply focused on this group alone. It is in these extreme cases that the rooting of negativistic reality distortion in the tendency to avoid emotional ambivalence becomes most convincingly evident. The low-scoring children, on the other hand, tend to refer more often to the individual differences between the children, sometimes with an explicit emphasis on the content of the first paragraph of the story which states that there are many kinds of children in this school.

The differential recall in the two groups of children is especially manifested in their answers to three more specific questions asked after they had written down their recall of the story as a whole. The last of these three questions called for a description of "the oldtimer who stuck up for the newcomers." Of all those failing to answer one, but no more than one, of the three questions, the percentage of high-scorers who omitted the last question was significantly higher than that of low-scorers. On the other hand, a significantly higher percentage of low-scorers omitted answering the second question which asked for the description of "the stoolpigeon," a boy with some undesirable characteristics.

Although the ratio of positive to negative characteristics mentioned by the low-scorers is closer to the one in the story, their emphasis on positive and nurturant aspects leads them sometimes to omissions. Gross distortions of the kind often found in the prejudiced group remain rare in their case, however. The fact that low-scorers on the whole approximate more correctly the actual configuration of stimuli will also be seen in the results of the perceptual experiments to be discussed below.

There are, further, still more general differences in the dealing with the story material which are of interest in this connection. The tendency to stray from the content of the story is in the high-scoring children combined with a tendency faithfully to remember certain single phrases and details. Some of the children in this group may show predominance of either the first or the second type of reaction. As in the Thematic Apperception Test, some will show a restricted approach concentrating on description of details of picture or story; others will go off altogether, telling stories which show almost no relation to the material presented. Thus there is either a clinging to the presentation with little freedom and distance, i.e., a stimulus-boundness in the sense of Goldstein as referred to above, or a neglect of the stimulus altogether in favor of purely subjective fantasies. It is in this manner that a rigid, cautious, segmentary approach goes with one that is disintegrated and chaotic, sometimes one and the same child manifesting both patterns in alternation or in all kinds of bizarre combinations. As does negativism and distortion in general, both these patterns help avoidance of uncertainty, one of them by fixation to, the other by tearing loose from, the given realities.

EXPERIMENTS ON PERCEPTUAL AMBIGUITY

We now turn to a group of experiments in perception proper. These are quite free from emotional and social content and are designed to help investigate whether or not such characteristics as intolerance of ambiguities are generalized. If such intolerance should turn out to be a formal characteristic of the organism independent of content, experiments on perceptual ambiguity could be used as diagnostic tools. We begin with preliminary experiments which showed a certain trend in the direction suggested in a relatively small number of subjects, although statistical significance was as a rule not scrutinized. This trend then was corroborated in further, somewhat modified experiments using more adequate numbers of subjects. In most cases the subjects were children in our project mentioned above who had scored extremely high or extremely low on ethnic prejudice. Unless otherwise specified, all the experiments discussed in this section were conducted between 1946 and 1948.

So long as experiments in perception dealt primarily with universal trends, the description of the sociological characteristics of the sample was comparatively irrelevant, or at least it seemed to be so. In entering the field of individual differences, however, the sociological attributes of the sample become decisive. Our own sample consisted mainly of a lower middle-class group in an area restricted by covenant. In samples of this kind the rigid extreme is apt to be strongly preoccupied with the maintenance of his or her middle-class status and of the social distance from the ethnic minority groups and from the unskilled workers living near by. The experiments on ambiguity reported in this section were to a large extent conducted with such extreme individuals within our sample; because of their tenuous social position, greater over-all rigidity is found here than in samples belonging to other classes or exhibiting a higher educational level.

Against the general objection which may be raised to the effect that intolerance of ambiguity is nothing but lack of intelligence, the following may be said. First, the correlations between absence of ethnic prejudice, on the one hand, and intelligence, on the other, are generally low. Second, there is no reason why rigidity could not be considered a malfunctioning of intelligence, although it would seem to be a rather specific aspect of intelligence that may be involved.

In the first of the preliminary experiments a disk-shaped reversible figure-ground pattern was presented to a total of 14 subjects. It was expected that prejudiced subjects would display a smaller number of spontaneous shifts, that reaction time in shifting would be longer, and that there would be a tendency to settle on one of the possible solutions. The answer to this may throw some light on the question as to whether subjects who exhibit rigidity in the emotional and social field are generally less likely to shift back and forth between alternative interpretations of an ambiguous perceptual configuration. The results of this experiment so far are not conclusive.

In another tentative experiment,[10] first the picture of a dog was shown, followed by a number of pictures representing transitional stages leading finally to the picture of a cat. At every stage the subjects were asked to identify the object on the given card. In spite of the fact that the cards were not too well drawn for the purpose, distinct trends became evident. The prejudiced group tended to hold on longer to the first object and to respond more slowly to the changing stimuli. There was greater reluctance to give up the original object about which one had felt relatively certain and a tendency not to see what did not harmonize with the first set as well as a shying away from transitional solutions. Once this perseveration was broken, there seemed to be in this group either a spell of haphazard guessing or a blocking by the uncertainties inherent in the situation. It may well turn out upon further evidence that intolerance of perceptual ambiguity is related to a broader psychological disturbance of which prejudice—itself often a deviation from the prevalent code, especially in school—is but another manifestation.

Turning again to less specific situations, figural aftereffects of what is, or has temporarily become, the "familiar" seem to show generally a relatively strong resistance to change in this group. It is as if any stimulus—or what seems to be "the" stimulus in the person's interpretation—is playing the role of an authority to which the subject feels compelled to submit. Situations which seem to be lacking in firmness are apparently as strange, bewildering, and disturbing to the prejudiced as would be a leader lacking in absolute determination. With internal conflict being as disturbing as it is in this group, there apparently develops a tendency to deny external ambiguity as long as such denial can be maintained. Underlying anxiety issuing from confusion of one's social identity and from other conflicts is apparently so great that it hampers individuals in this group in facing even the purely cognitive types of ambiguity. The mechanism discussed is somewhat related to what Postman, Bruner, and McGinnes (27) have called "perceptual defenses." A desperate effort is made to shut out uncertainties the prejudiced individual is unable to face, thus narrowing what Tolman (34) has called the "cognitive map" to rigidly defined tracks. Persons with less severe underlying confusions, on the other hand, may be able to afford facing ambiguities openly, although this may mean an at least temporary facing of conflicts and anxieties as well. In this case the total pattern is that of a broader integration of reality without shutting off parts of it, and thus a more flexible adaptation to varying circumstances.

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[10] The use of gradual transitions between objects of different kind occurred to the writer in reading Goldstein's (**15**, p. 309) description of certain schizophrenic patients who insisted on assigning "individual words" to each in a series of shades of green. Although the schizophrenics differ from the ethnically prejudiced in the degree of reality disturbance, the two seem to have in common a tendency to absolutize, or to absorb at one end of the scale, or to discard entirely differences which others will be able to integrate into a continuum of gradual steps. Relatively unstable stimuli representing intermediate stages between more clearcut configurations have been common in experiments on the perception and memory-distortion of form for some time. For suggestions regarding one of the experiments reported here the writer is indebted to Dr. Warner Brown.

Drawings representing transitions between objects more drastically different from one another than those mentioned above, such as between a tree and a house, seem to be more suitable for our purpose.

In a further experiment in which one after another in a progressive series of hues was to be named, the writer gained the tentative impression that prejudiced subjects again perseverate longer than the unprejudiced, in this case with a given color-term, conceive of fewer and cruder steps along the scale, or tend toward one-dimensional rather than more complex systems of classification.

Another experiment, conducted recently along the general line of the one with the cat-dog pictures just mentioned, is one by Mr. Norman Livson and Mrs. Florine Berkowitz Livson. Numbers were used rather than objects, thus reducing the possibility of involvement with content. Two statistically significant differences were found (both at the 1 per cent level of confidence) for a total of forty-two of the children in our social discrimination project. One is the relatively slow recognition of numbers emerging from indistinctness, the other the relatively slow recognition of numbers changing from other numbers, by the ethnocentric group. Again, there was a prolonged clinging to the first impression, even though faulty, on the part of the prejudiced.

Rokeach (29) investigated a problem of rigidity related to ambiguity. He used a gestaltpsychological thinking problem involving the manipulation of three jars. A mental set was first established by presenting the subjects with a series of problems which could be solved only by a relatively long and complex method. The subjects were then presented with further problems which could be solved either by maintaining the original set or by using a more advantageous direct and simple method. A measure of rigidity was derived from the number of cases in which the established set was maintained and thus an inability demonstrated to restructure the field and to perceive the direct solution. The results presented by Rokeach indicate clearly that the children scoring extremely high on ethnic prejudice solve the new problems more rigidly than those extremely low on prejudice. Over and above the measure of rigidity, based on maladaptive perseveration, Rokeach utilized the amount of scratch paper used as an aid in solving the problems as a measure for concreteness of thinking. Those high on prejudice were found to use more scratch paper than those low on prejudice. A spatial problem also devised by Rokeach, calling for the finding of a shorter path on a map after a set for a longer route had been established, has further borne out the greater rigidity of the ethnically prejudiced.

The fact that the sample of children from our project on social discrimination who were used as subjects in the experiments of Rokeach had been studied with respect to a variety of personality characteristics made it possible to interrelate different measures of rigidity. Thus the rigidity scores derived from the simple arithmetic problems just referred to tend to correlate with over-all clinical ratings of children's rigidity based on their attitudes toward parents, sex roles, self, moral values, etc., as revealed in our clinical interviews.[11] Similarly, significant correlations were found between rigidity scores gained by Rokeach and the total score on the personality inventory. This personality inventory had been designed to measure dichotomizing in emotional and social attitudes. It consists of a series of statements referring to attitudes toward authority, aggression, weakness, etc. A few statements from this inventory have been quoted above.

As mentioned earlier, intolerance of ambiguity must further be related to a reluctance to think in terms of probabilities and a preference to escape into whatever seems definite and therefore safe. Murray E. Jarvik adapted a technique developed by Egon Brunswik in a probability discrimination experiment with rats in which food rewards had been distributed with differing relative frequencies on the two sides of a simple T-maze (4). Jarvik presented a long series of pictures of white and Negro children to our ethnically prejudiced and unprejudiced children, asking them to state in each case whether they were dull or bright looking. After each response, the supposedly correct answer was given by the experimenter. For half of the children 75 per cent of the whites and 25 per cent of the Negroes were designated as "bright," and the rest as "dull," and vice versa for the other half of the children. A preliminary inspection of the data shows that the children extremely low on prejudice caught on to this probability learning situation more readily than those high on ethnocentrism; the latter tended to persist in their preconceptions, being less able to absorb the general trend of the information given.

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[11] The evaluation of these interviews was undertaken by Miss Joan Havel in collaboration with the present writer. A joint publication on the clinical interviews and their relationship to social ideology is in preparation.

Some of the traits mentioned in this section may be more fully ascertained by the use of such projective tests as the Rorschach, especially when the slant of interpretation given to these tests by Rapaport (28) is applied. Aside from a certain advantage of setting up specific experiments for specific variables, perceptual experiments of the kind described ordinarily offer a "reality" more clearly delineated, whereas projective techniques are purposely being kept vague in a greater variety of directions. Since we were in the present context more interested in the handling and mastery of a well-circumscribed reality, and less in projections as such, the experiments listed have an edge over projective tests for our particular purpose.

Though the over-all trend of the data discussed in this section seems to indicate a certain generality, within the individuals concerned, of the approach to reality which we have subsumed under the term "intolerance of ambiguity," a much wider array of both techniques and population samples would be necessary to establish this generality with an adequate degree of definiteness. It must further be kept in mind that, as is being pointed out elsewhere in this paper, compensation of rigidity by—often exaggerated—flexibility is probably as much present as is positive correlation between different aspects of rigidity.

Detailed inspection of case studies which include both clinical data and data from experiments on such cognitive topics as preception, memory, and problem solving show consistencies as well a apparent inconsistencies in this respect. For instance, there ar perhaps some exceptional cases of children who are extremely hig on the prejudice scale but manifest only an average amount of mental rigidity. The discussion of such inconsistencies would easil fill another paper. We shall give here only some indication of th complexity of the facts involved. In one of the inconsistent case of the kind just described a boy's ethnic prejudice turned out t stem mainly from a marked physical marginality. His mother wa loving and permissive, and her method of child training fitted bette to the boy's relative mental flexibility than to his high prejudice On the other hand, a girl who was most articulate in proclaimin a liberal ideology and who at the same time displayed more menta rigidity than is common in the unprejudiced turned out to be member of a family which, though clinging in a dogmatic an militant way to a liberal ideology, did so with a great deal of inexorability and a lack of willingness to arbitrate with, or to accept those who thought differently.

Rigidity, Discontinuity, and Chaos: the Closeness of Opposites

There is evidence, as pointed out above, for the fact that rigidit of attitudes constitutes a counterbalance to underlying conflicts ofte verging on chaos. Rigidity and chaos seem on theoretical as we as on empirical grounds closer related than one may assume fro a purely phenomenological viewpoint. Goldstein (15) has pointe out the coexistence of rigid and disintegrated behavior in the sam individual.

A combination of rigidity and chaos similar to that found i the cognitive and in the strictly emotional field is also present in th social field. In our own study the same children who adhere to th status quo and resist change tend to subscribe to statements whic predict catastrophes, chaos, a total change in which everything is i flux. Examples of such statements are:

> If everything would change, this world would be much better.
>
> Some day a flood or earthquake will destroy everybody in the whol world.

In fact, we find in the rigid children artificial isolation and separation of feelings and attitudes which belong together and at the same time fusion of attitudes which do not belong together. Apparently this is the way these children become restricted, in their awareness, to a small segment of their reactions to authorities, to the other sex, and to themselves. Much of the feelings which actually originate in their relationship with the parents is not seen in the original context but displaced onto political leaders, minority groups, foreign countries, and so forth, leading to a personalization of the entire social outlook. There is on the surface a clinging to the ethnic, social, and sex identity. As may be seen especially from the indirect material, there is at the same time confusion about one's identity along all those lines. Underlying identification with, and envy of, the other sex as well as the out-group with its alleged pleasures and emotionality are clearly manifest.

It should be mentioned here that Werner (36) conceived of "rigidity" as being caused by either too much isolation of the different subareas of the personality or by too much overlapping in the sense of a perseveration of certain elements throughout the entire mental life. Both the isolation and the keeping apart of sets as well as the tendency toward undue perseveration of the same set can serve the function of avoidance of complexities.

Another pair of attitudes closely connected with our topic is *concreteness* vs. *abstractness*. It has been described in great detail by Goldstein and Scheerer (16). The concrete attitude is one of being "bound to the immediate experience of the given thing or situation in its uniqueness." It can easily be seen how the escape into the concrete, the "boundness" to the specific stimulus, represents dealing with tangibles and certainties where everything opaque and complex can be avoided. We would like to add that too crude generalization and the inclination toward mechanical repetition of faulty hypotheses—an experiment on the latter has been referred to above—can serve the same function. Adequately flexible organization of the psychological field, as contrasted with orientation toward concrete detail with its close relation to disintegration and chaos, has likewise been discussed previously in this paper. Such pairs of cognitive opposites as concreteness vs. abstractness, or the related one of objectivity vs. subjectivity, must be reviewed in a somewhat new light to the effect that extreme objectivity and extreme subjectivity are concurrent rather than mutually exclusive attitudes.

The splitting off and exclusion from consciousness of unacceptable tendencies such as aggression against authorities, fear, or weakness, may be considered as contributing to the general lack of insight, the rigidity of defense, and the narrowness of the ego characteristic of those intolerant of ambiguity. It must lead, furthermore, to all kinds of discontinuities as revealed especially in the attitude of the rigid person toward himself. We refer here to the occasional breaking-through of an aggression, fear, or weakness of which there has been no awareness and which may have been attributed to someone else. Such a person attempts to keep these tendencies from interacting with other tendencies and with reality.

Another discontinuity concerns the subject's notion of development. The intolerant individual makes significantly fewer spontaneous comments on his own early childhood; upon inquiry we discover that a discontinuity between present and past self often becomes apparent. In the attitude toward one's sex role and parents, similar discontinuities have become evident. Verbalized love for the parents or an explicitly stated masculinity are contradicted by tendencies in opposite directions observed within the same material (see **1**, Chapters X and XI).

It is apparently the great number of conflicts and confusions present in the prejudiced which leads to their resorting to black-white solutions. Too much existing emotional ambiguity and ambivalence are counteracted by denial and intolerance of cognitive ambiguity. It is as if everything would go to pieces once the existing discrepancies were faced. To avoid this catastrophe everything that might abet the uncertainty and opaqueness of life is desperately avoided by a selection of undisturbing, clear-cut, and therefore too general or else too concrete aspects of reality. Greater rigidity of defenses is necessary to ward off the danger of becoming completely overwhelmed by the repressed forces.

These considerations also show that intolerance of ambiguity is intrinsically equivalent to an oversimplified and thus reality-inadequate approach, characterized by the dominance of crude, relatively unessential aspects, and often combined with glaring omissions of fact. As long as we remain in the emotional and especially in the social field the evaluation of what is a reality-adequate or a reality-inadequate representation might be hard to judge. The fascist may accuse the liberal and the liberal the fascist of distorting reality. It seems very probable on the basis of our knowledge that our prejudiced subjects are mistaken about the existing relationships between ethnic group membership and certain personality traits. The least that can be said is that they exaggerate existing correlations and that they are frequently mistaken about the explanations of such correlations. On the other hand, a certain sub-type of liberal tends to do the opposite, denying the existing relationships altogether, and with this implicitly the importance of the significance of social factors, such as membership in a minority group. All in all, we have the impression from our data that the liberals distort reality less. In order to demonstrate this, a shift to emotionally more neutral cognitive material had to be effected. The experiment on memory discussed above bears directly on this point, confirming our expectation.

The subtle but profound distortion of reality in the course of the elimination of ambiguities is precipitated by the fact that stereotypical categorizations can never do justice to all the aspects of reality. So long as our culture provides socially acceptable outlets for suppressed impulses, smooth functioning and fair adjustment can be achieved within the given framework. It must be kept in mind that the adjustment of this type of person depends on conditions which are comparatively narrowly circumscribed. Whenever differentiation and adaptability to change are required, this adjustment will run the risk of breaking down. Basically, therefore, avoidance of ambiguity and related mechanisms, directed as they are toward a simplified mastery of the environment, turn out to be maladaptive in the end.

The precise, machine-like, unswervingly unambiguous perceptual reaction glorified by Jaensch (17, see also above) can thus be regarded as no more than a mixed blessing. In fact, many of the perceptual reactions of highest fidelity which seem so ideal to Jaensch must be considered as a rigid, if not pathological, stimulus-boundness in the sense of Goldstein. Jaensch's favorite "integrated" type is in reality one who makes excessive use of the mechanism of isolation rather than one who sensibly interconnects different spheres of reality. It certainly is only a very superficial kind of integration a rigid person can achieve. On the other hand, proneness to interconnect the seemingly diverse by no means always constitutes an undue mixing-up of issues; more often than not it will represent a creative establishment of essential relationships.

A case in point against Jaensch is the gross distortion of reality, such as overestimation of one's own strength and underestimation of that of the enemy as exemplified in Nazi ideology and behavior, with an utmost precision in work on technological detail. Fidelity in small matters often goes hand in hand with gross errors in the understanding of the most essential aspects of reality. Obviously, on the other hand, persons capable of creative efforts are bound to have small matters mechanized at the risk of being off the right track in a certain proportion of instances.

Much in the analysis of our material undertaken in the present paper may also be summarized in the statement that it is probably neither extreme stimulus-boundness nor extreme subjectivity which is really helpful in understanding the world, but rather the classifying of concrete experiences under over-all guiding principles. There is ample evidence from various kinds of clinical material that the clinging to external stimuli is often a sign of anxiety, a "way out from the organism's unbearable conflict as a protection against the danger of severe catastrophe" (Goldstein, **15**). By contrast, the categorial or conceptual attitude is characterized by the ability or readiness to assume a mental set voluntarily, to shift voluntarily from one aspect of the situation to another, to keep in mind, simultaneously, various aspects, to grasp the essentials of a given whole, to break up a given whole into parts and to isolate them voluntarily, to abstract common properties, to plan ahead ideationally, to assume an attitude toward the "merely possible," to think or perform symbolically, and finally to detach our ego from the outer world.

To point out the fact that the extremely concrete, fixated orientation leads to disintegration of human behavior, Goldstein (**15**) discusses the thinking of the schizophrenics and points out that this concreteness does not prevent the entering of subjective, changeable ideas and thoughts, but that this type of patient deals with ideas as if they were concrete things which belong to an object or situation. There is "an inability to maintain adeqate boundaries," especially between the ego and the outer world.

Here we would like to stress, even more than Goldstein himself does, the paradoxical combinations which are found in the impaired reaction of, say, the schizophrenic. The schizophrenic is not only very concrete but at the same time very general. That is, he transfers a specific property of a stimulus to many other stimuli. This tendency toward generalization may be connected with perseveration and may be due to the overlapping of different areas of personality. With Werner (36) we may see in this overlapping, as well as in isolation, causes for rigidity. We thus might find rigid concreteness occurring together with rigid generalization; both have to be distinguished from an adequately abstract approach.

We may further point out in support of this that while Goldstein consistently stresses the concreteness of the schizophrenic, Kretschmer (22) thinks of the schizoid primarily as given to abstraction, while it is its opposite, the cycloid (related to the manic-depressive), who is seen to be more concretely oriented. The greater ability for abstraction on the part of the schizoid type is, among others, said to be manifested in his ability to concentrate more fully on certain characteristics of the stimulus to the neglect of all others. If the task is to reproduce colors he will succeed in doing so, whereas the cycloid will tend to recall letters along with colors. The schizoids are declared to be superior in sorting and parceling out, whereas the cycloids tend toward global and total impressions, concentrating on concrete objects as a whole. The free associations of schizoids to a given word are more logical in the formal sense, whereas those of the cycloids are more affective, more direct, and more fluent. In all, the thinking of the cycloid is seen to be less precise and logical than the thinking of the schizoid; the cycloid is said to show more realism, more love of detail, less differentiation between relevant and irrelevant features.

Whereas in the present study the main emphasis was on environmental factors, Kretschmer stresses constitution as a determinant of differences in cognitive and emotional behavior. Our evidence regarding environmental factors must not be allowed to detract from the possibility of constitutional differences in rigidity and frustration tolerance, although they may well turn out to be of a different kind than those emphasized by Kretschmer.

It is interesting to observe that Kretschmer seems to interconnect ability to abstract with autism. As he points out, the schizoid ignores in his abstractions what seems not to fit. Although Goldstein is far from being blind to this fact, he still stresses more the connection between concreteness and autism—rather than that between abstraction and autism—and seems to think that they tend to occur together in the same individual. Evidence presented in this paper suggests that extreme concreteness and extreme generalization may well go hand in hand and are both relatively primitive forms of reaction.[12]

Realization of the possibility that apparent opposites such as extreme abstractness and extreme concreteness, or extreme subjectivity and extreme objectivity, may be psychologically closer together and more apt to combine with each other in the same subject than any of them would with an intermediate position along the same scale may be traced back to psychoanalysis. To be sure, in psychoanalysis this pattern is discovered primarily in the field of emotion, through the phenomenon of ambivalence. Conscious love of extreme and exaggerated intensity is viewed with the same suspicion by the psychoanalyst as are extreme feelings of hate.

One of the characteristics of the perception-centered outlook of most personality psychologies based upon cognitive categories is that they tend to ignore this fact and to stick to the more obvious policy of naïve dichotomizing in which opposite extremes are conceived of as true polarities.

It is only where work as been centered about the study of normal and abnormal personality organization that the closeness of opposites and the juxtaposition of the intermediate as against either or both of the extremes begins to make itself felt along with that between one extreme as against the other. Personality-centered research would, at least to the present writer, seem to be bound to give great prominence to the idea of the closeness of opposite extremes. Results of research designed with an eye on genuine problems of personality dynamics will of necessity force upon the investigator the recognition of this basic relationship. Thus in studying mechanisms of self-deception (7) the writer found that exaggerated verbal emphasis on certain ideals of conduct and related extremes of protestation and of other linguistic expression were indicative of shortcomings rather than of strength with respect to the traits in question in the subjects concerned. In quite different context (9) it was shown that the need for aggression may either be openly expressed—one extreme—or else lead to general tenseness with a minimum amount of overt aggressiveness—opposite extreme within a system of "alternative manifestations." If exaggerated aggressiveness is openly manifested, this in its turn correlates often with indications of a need of an opposite kind, for instance, the need for dependency.

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[12] For animals the relationship between organically induced mental deficiency, on the one hand, and a decrease in the variability of behavior which is one of the most outstanding characteristics of the concrete attitude, on the other, has been described by Krechevsky (**21**).

An interesting parallel to the phenomenon of grossly alternative behavioral manifestations just referred to has been described for the physiological domain of the bodily expression of emotions by Harold E. Jones (18).

It must be emphasized here that expression of genotypically identical needs or drives through alternative manifestations which are phenotypically far apart from, or even opposite to, each other by no means detracts from the possibility of the generality of a certain basic personality feature. Thus rigidity in one respect may go with flexibility in another; yet they both may actually be part of a dynamically coherent personality make-up. In fact, it may even be impossible—in an almost logical sense of the term—to remain extremely rigid, or objective, or concrete, without at the same time making up for it by extreme flexibility, subjectivity, or abstractness in some other respect compensatory to the first. The problem as to whether individuals differ primarily with respect to over-all rigidity or with respect to the differential distribution of rigidity within the various aspects of personality thus appears in a new light and acquires even greater urgency.

Uniformity of style of a person in the sense just described would then be defined in terms of such formal characteristics as, say, being extreme or given to exaggeration in any direction within any or all dimensions of conduct. Such concepts as ambivalence or fear of ambiguity, being at least in part descriptive of certain aspects of the closeness of extremes, are thus automatically included with these formal characteristics. On the basis of the earlier study of self-deception mentioned above (7) as well as of the evidence on the ethnically prejudiced referred to throughout this paper, one may indeed be led to believe that the formal elements of personality style are not in the same manner subject to censorship—in the psychoanalytic sense of the term—as is the more concrete content of wishes and instinctual tendencies. Since it is only the content which is directly threatening, the form in which it is set may not seem in need of being censored. If this holds true, formal style elements, such as exaggeration, easy generalization (as contrasted with salient specificity), intolerance of ambiguity, and the like, would be of greater penetrating power within the personality, more nearly alike on the surface and at greater depth, and thus of greater generality and greater diagnostic validity than such content elements as, say, sex, aggression, the Oedipus complex, and the like.

Conclusion

In the present paper, an attempt was made to discuss denial of emotional ambivalence and intolerance of cognitive ambiguity as but different aspects of what may be a fairly coherent characteristic. An underlying emotional conflict between glorification and hostility in the attitude toward parents, sex, and one's own social identity previously demonstrated in children inclined toward rigid social dichotomizing as revealed by ethnic prejudice is taken as the impetus for experiments in memory, perception, and related topics, devised to test tolerance of ambiguity on an emotionally more neutral ground. There is some indication of a prevalence of premature reduction of ambiguous cognitive patterns to certainty in the prejudiced subjects, as revealed by a clinging to the familiar, or by a superimposition of one or many distorting clichés upon stimuli which are not manageable in a more simple and stereotyped fashion. There is some indication that in the case of distinct intolerance of emotional ambivalence one may as a rule be able to locate at least some aspects of intolerance of cognitive ambiguity although these may often be more apparent on a higher level than that of perception proper.

More detailed study of single cases, however, reveals that social and cognitive intolerance are far from being inseparable even in the extreme groups. Furthermore, it seems likely that in specific contexts flexibility in one perhaps essential respect may have to be compensated for by rigidity in other perhaps relatively unessential respects. On the other hand, the clinging to the familiar and precise detail can go hand in hand with the ignoring of most of the remaining aspects of the stimulus configuration, resulting in an altogether haphazard type of approach to reality. (The over-all equivocal relationship between emotional and cognitive factors and abilities has been discussed by the present writer elsewhere, 8).

Emotional ambivalence, and related reciprocities of the kind just referred to, help to imbue personality-centered study of cognition as exemplified in this paper with the realization of such important dynamic principles as the closeness of opposites. Thus lack of distance and too much distance to culture, parents, and other stimulus configurations seem to be more closely related to each other than is either of these two extremes to what we may term a "medium distance" from these situational elements.

From these complexities and compensations let us turn once more to positive correlation. In general, severe repression of certain tendencies such as aggression toward authority, fear, weakness, or elements of the opposite sex in oneself, find their parallel in an externalized image of these tendencies as projected onto others and in a narrowness and rigidity of consciousness. There is more than an empirical affinity between the strength of hostility, of power-orientation, of externalization, and of rigid stereotyping, on the one hand, and the intolerance of ambiguity, on the other; there is a similar affinity between the orientation toward love and the acceptance of drive-impulses, on the one hand, and a general flexibility, on the other. The struggle between these two orientations is basic to our civilization; its individual members display these two patterns in varying proportions and changing configurations.

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PERCEPTION AND PERSONALITY

A Symposium

PART II

THE NATURE AND IMPORTANCE OF INDIVIDUAL DIFFERENCES IN PERCEPTION

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INTRODUCTION

The approaches used in the study of problems of perception have traditionally been of two main kinds. In one sort of approach the explanation of perceptual experiences has been sought in the structure of the prevailing field. In the other the emphasis has been upon the operations of the sense organs and associated neural structures mediating the stimulus, as well as upon the accumulated effects of the person's past experiences with the stimulus. Recently, on the basis both of specific experimental findings and of general theoretical consideration, there has been a questioning of these approaches, especially because of their failure to consider the role of motivational factors in perception. This development is significant for two reasons. First, it leads to consideration of perception in a broadened context, as a process that can be fully understood only in relation to the over-all psychological organization of the person concerned. Second, it suggests that the use of controlled perceptual techniques may provide a means of understanding and diagnosing personality. Not only is this a relatively unexploited approach to personality study, it is also a fairly objective one.

Probably the most impressive evidence, heretofore, of the role of personal factors[1] in perception has come from studies employing ambiguous situations, such as the Rorschach Test. This is not at all surprising. In ordinary experience the perceptual situations with which the person is confronted range in a continuum from highly ambiguous to well-structured ones, with the latter occurring more frequently. As one progresses through this continuum, personal determinants become less important and perception is increasingly dictated by the structure of the situation. It is therefore to be expected that the readiest evidence of the importance of personal factors in perception should be derived from ambiguous situations, where, in the absence of any compelling organization in the material itself, the person has greater opportunity to structure it in his own unique fashion. There is a commonly held view, however, that in a well-structured situation perception is determined entirely by the character of the field, with personal determinants playing little if any role. This conception implies that personal factors are not usually important in perception but become important only in the very special circumstance of the ambiguous situation. To form a comprehensive estimate of the role of personal factors in perception, therefore, it becomes especially important to examine perceptual processes which ordinarily occur under well-structured conditions. This has been done in the experiments described below.

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[1] The terms "personal factors," "personal determinants," or "individual determinants" will be used here broadly to refer to general psychological characteristics of the individual (motivational, cognitive, emotional) that may influence his perception.

Individual Differences in Mode of Space Orientation

For some time our laboratory has been engaged in a systematic investigation of personal determinants of perception. This work has for the most part been concerned with the nature and basis of individual differences in a particular though broad process: namely, perception of the orientation of objects, including one's own body, in relation to the upright in space. This section will describe some results of this investigation, as well as the methods employed.

Problem and Methods

The ability to establish the upright as quickly, effectively, and accurately as we ordinarily do is based upon the stable and compelling representation of the upright in our surroundings and upon our possession of adequate sensory equipment for its detection. First, the direction of gravity, which corresponds to the true upright, is readily apprehended through the continuous postural adjustments made to this force. Second, the visual environment has the character of a framework, whose main outlines represent the true vertical and horizontal; and this also provides a ready basis for establishing the upright. The co-operation between these two sets of factors is ordinarily very intimate, resulting in a unified impression of the upright; and, furthermore, since the visual and gravitational uprights coincide in direction, the outcome is the same whichever determinant is used as the main basis of perception. This perceptual process is of great and continuous importance to the person, since it involves his body directly and enters constantly into his adjustments to objects in the environment. These and other characteristics to be discussed later make it especially useful in the study of personal factors in perception.

To investigate individual differences in manner of establishing the upright, three standardized test situations were developed. Use was made of knowledge obtained in earlier studies, in which the roles of various structural characteristics of the situations and of the main sensory determinants involved had been carefully worked out.[2] Attention was also given to the following considerations:

(1) Under ordinary conditions, as noted, the visually and posturally indicated uprights coincide in direction, so that judgment of the upright on the basis of one or the other, or both, leads to exactly the same result. Hence, even if people do differ in manner or extent of dependence on one or the other standard, such differences cannot ordinarily be detected. To detect them, it is necessary to separate these standards experimentally. This was done in the first two test situations described below by tilting the visual frame-

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[2] Before a perceptual situation is applied to the study of personal determinants of perception, it must be carefully investigated, in order to establish the roles of other types of variables that influence performance in it. Unless the effects of these variables, and in particular their contributions to individual differences in performance, are known and controlled, it is not possible to presume that differences among subjects reflect differences in the operations of personal factors. Such an advance understanding of the perceptual situation is especially necessary, since perceptual processes are typically quite "sensitive" in that they are readily affected by slight changes in the conditions under which they are produced. Thus, it is not unusual for a slight modification in the instructions used in defining the subject's task to lead to important changes in results.

It may be noted here too that often even well-known and long-used perceptual techniques cannot simply be taken over as they are for use in the study of personal factors in perception. To make them reliable and valid instruments for testing the perception of individuals, or differences among individuals, they typically require very considerable development and adaptation.

The three tests discussed here are described in greater detail in a forthcoming paper by the writer: "The nature and measurement of individual differences in space orientation." The earlier experimental work with the situations used in these three tests is described in the following reports: H. A. Witkin, "Perception of body position and of the position of the visual field," *Psychol. Monogr.*, 1949 (in press); H. A. Witkin and S. E. Asch, "Studies in space orientation. IV. Further experiments on perception of the upright with displaced visual fields," *J. exp. Psychol.*, **38**, 1948, 762-782; H. A. Witkin, "Perception of the upright when the force acting on the body is changed," *J. exp. Psychol.*, 1950 (in press); H. A. Witkin, "Further experiments on perception of the upright when the force acting on the body is changed" (in preparation).

work while the gravitational upright remained unaltered. In the third test situation the separation was accomplished by altering the direction of the gravitational force acting on the body while the visual framework remained upright. With both types of change adherence to the visual framework results in a different location of the perceived upright than does adherence to the posturally experienced gravitational upright; and this permits evaluation of the extent of an individual's dependence on each factor.

(2) The task of determining the direction of the upright presents itself in the following different, though not necessarily separate, ways. (a) The direction of the field as a whole must be determined—for example, whether the cockpit of a plane, in a fogbank which obliterates the earth below, is level. (b) The direction of an object within the field must be determined—for example, whether a picture on the wall of a room is straight. (c) The position of the individual's body in relation to the upright must be determined—for example, whether it is erect or tilted at a given moment. These tasks are represented in the three tests described below—(a and c) in the first and third tests, (b) in the second test. These tests together present the person with essentially all conditions under which orientation toward the upright may be required.

(3) In order to evaluate a given person's perception in various situations, or to determine even small differences among people in a given situation, it is desirable that the subject's perception be expressed in quantitative terms. The alternative procedure of simply obtaining from the subject a report of his impressions would make such evaluation and comparison extremely difficult. To translate the subject's perception into a quantitative result, each of these tests required some *measurable* action from him: he was called on to make changes in the position of the visual framework, or of his own body, or of another object in space. The assumption was made, of course, that the action in these circumstances corresponded directly with the subject's perception of position, and that differences in action reflected differences in perception.

The Perceptual Tests

1. *The tilting-room-tilting-chair test.* This test evaluates the individual's perception of the position of his body, and of the whole

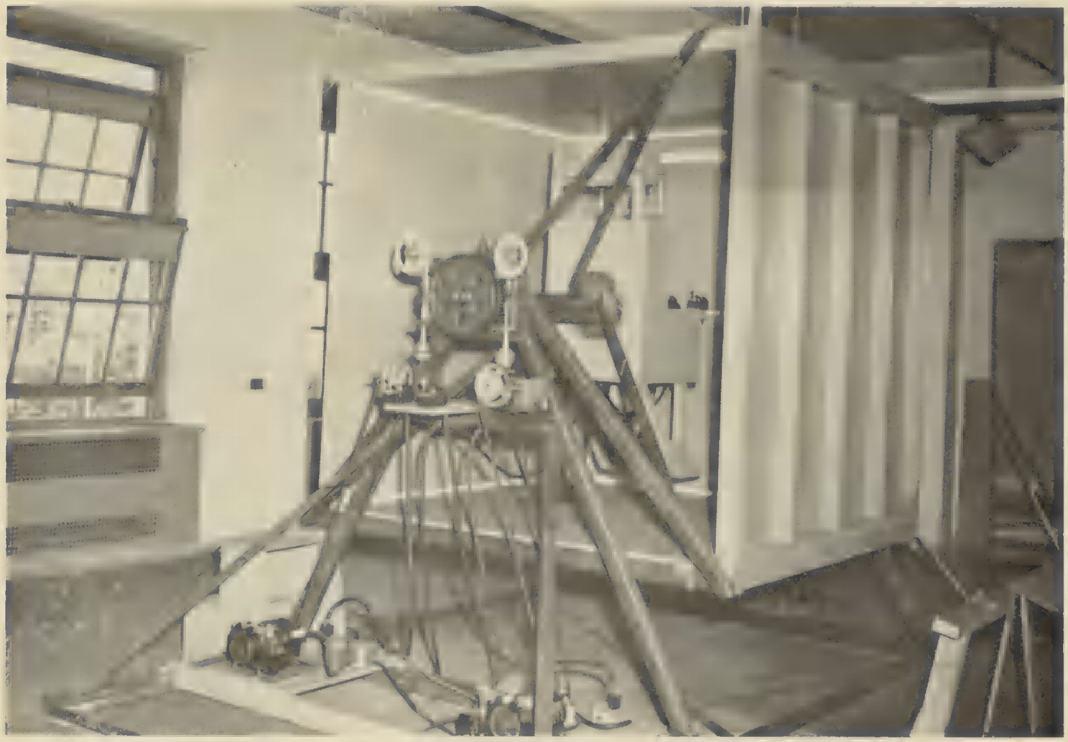

Fig. 1. The tilting-room-tilting-chair test.

surrounding field, in relation to the upright. The apparatus used is shown in Figure 1. It consists of a small room within which is a chair. Each may be tilted to left or right, by any amount, either by the experimenter or by the subject seated in the chair. Since room and chair are provided with independent driving mechanisms, they may be tilted alone or together, to the same side or opposite sides, at the same speed or different speeds. The subject, seated in the chair, cannot see outside the room in which he is contained, so that his judgments of the upright must be based on whatever information he can obtain from his own body and from the visual field of the experimental room. The test procedure consists of tilting the room and chair to set positions, and then requiring the subject to adjust one or the other to the upright—the room on some trials, his chair on other trials. The positions to which room and chair are brought may be measured to the nearest degree, so that the results are expressed in quantitative terms.

The standard test consists of four parts. (a) Series 1: Room and chair are initially tilted to the same side (room at 56° and chair at 22°), and the task is to adjust the room to the upright. (b) Series 2: Room and chair are initially tilted to opposite sides (room at 56° and chair at 22°), and again the room must be adjusted to the upright. (c) Series 3a: Room and chair are initially tilted to the same side (room at 35° and chair at 22°), but now the chair must be brought to the upright. (d) Series 3b: Room and chair are initially tilted to opposite sides (room at 35° and chair at 22°), and again the chair is brought to the upright. Four trials are given in Series 1 and 2, and three in Series 3a and 3b. Series 1 and 2 both involve adjustment of the room, and Series 3a and 3b adjustment of the chair.

2. *The rod-and-frame test.* This test evaluates the individual's perception of the position of an item in a field. The apparatus used consists, first of all, of a luminous square frame, pivoted at its center so that it may be tilted to left and right. Pivoted at the same center, but moving independently of the frame, is a luminous rod. Since the test is conducted in a completely darkened room, all the subject can see is the frame and the rod. These are presented in tilted positions, and the subject is required to adjust the rod to the true upright while the frame remains in its initially tilted position.

As a further variable, the subject's body is upright during some trials, tilted during others. It is, of course, much more difficult to make use of one's body in judging the position of a distant line when the body is tilted than when it is erect. In this test, therefore, it is possible to determine the extent to which the subject is able to make use of postural experiences in overcoming the influence of the frame, both at times when it is easy to use these experiences and at times when it is difficult.

Three series, each consisting of eight trials, comprise the standard test. (a) Series 1: The subject is tilted to one side (at 28° left or 28° right) and the frame is tilted to the same side (at 28° left or 28° right). The rod is to be adjusted to the upright from an initial tilt of 28° (at times to the same side as the frame, at other times to the opposite side). (b) Series 2: Body and frame are tilted to opposite sides (both at 28°). (c) Series 3: The body is erect and the frame is tilted to left or right (at 28°).

3. *The rotating-room test.* This test evaluates the subject's perception of the position of his body and of his surroundings when the direction of the force acting on his body has been changed. In the two previous tests the procedure followed was that of tilting the field and tilting the body. Whereas tilting the field does fundamentally change the direction of the visual upright, tilting the body does not at all change the direction of the gravitational upright. Though more difficult than with body upright, it is still possible when the body is tilted to detect the direction of gravity through the bodily sensations arising from postural adjustment to this pull. The rotating-room test, however, by actually altering the direction of the force acting on the body, changes the postural factors in a more fundamental way. To alter the force on the body, an outward-acting centrifugal force, produced by rotating the subject, is added to the downward pull of gravity, yielding a resultant with a direction intermediate between the two forces.

The apparatus employed consists of a small, fully enclosed room, mounted on a carriage that is driven about a circular track. This room may be tilted from side to side, and contains a chair that may also be tilted to one side or the other. The rotation of the whole unit and the tilting of room and chair are controlled by the experimenter from the outer laboratory.

The standard test consists of two parts, in each of which four trials are given: (a) Series 1: Room and chair remain in their initial objectively upright positions, and the subject's task is to make the room straight if it appears tilted to him. (b) Series 2: Again room and chair remain in their initial objectively upright positions, and now the subject's task is to make his chair straight if he perceives himself as tilted.

The Nature of Individual Differences in Mode of Orientation

Studies with the situations used in these three tests have established the general principles governing orientation toward the upright. The following discussion will be limited, however, to those findings that are related to the problem of individual differences in perception.

Though presenting a variety of conditions, the three test situations have an important feature in common: they all involve a conflict between the main (visual and postural) determinants of the perceived upright. Therefore each presents the problem of integrating these conflicting sensory experiences and arriving at a unified perceptual impression. The results clearly show that in all the test situations subjects differed markedly in how they resolved the conflict and consequently in how they perceived the situation. Specifically, they differed with regard to the relative emphasis assigned to visual impressions and to bodily experiences in the final integration. Whatever the outcome, a given way of perceiving the situation represented in almost all cases an automatic and spontaneous experience for the person. Thus in the tilting-room-tilting-chair test, with the structure of the situation identical for all subjects, the room (tilted at 56°) immediately *looked* straight to some, *looked* mildly tilted to others, and *looked* very much tilted to still others. The same kinds of differences were found in the way the subjects perceived their own body position. These findings indicate that even a fairly well-structured situation will not be perceived in the same way by everyone. The discovery of such differences, of course, is dependent on the use of appropriate experimental procedures.

The nature and extent of the observed differences in perception may be illustrated with some specific results for each of the three tests. First to be considered is the tilting-room-tilting-chair test, and in particular that part of the test in which the subject was required to adjust the room to the upright (Series 1 and 2). Most often in this situation subjects tended to "go along with the field," so that the perceived upright was shifted toward the vertical of the tilted room. Some subjects simply identified the true upright with the vertical of the room, perceiving the room at the outset as fully erect at its initial 56° tilt. Others located it somewhere between the vertical of the room and the vertical indicated by experiences of body position, and moved the room from its initial tilt to some position closer to the true vertical. Still others, few in number, were successful in bringing the room to the true upright, indicating that they judged the upright independently of the field and in accordance with the position of the body. In Series 2, for example, where room and body were both initially tilted to opposite sides and the task was to straighten the room from its 56° tilt, the average scores (representing the objective tilt of the room when it was judged to be upright) were 22.9° for men and 28.1° for women. Examples of extreme performance were found at both ends of the range. One subject, a man, succeeded in bringing the room to within an average of 2.5° of the upright in the four trials of this series; another subject, a woman, perceived the room as upright at its initial 56° tilt on all four trials.

Equally striking differences were obtained in that part of the test where the subject's perception of the position of his own body was evaluated (Series 3a and 3b). When required to straighten the body, with the room tilted, most subjects moved it in the direction of the tilt of the room, indicating that they based their judgments not only on how the body felt, but also on how it looked—i.e., whether or not it was aligned with the surrounding field. (The average amount of tilt of the subject's body when it was judged to be upright was 8.2° for men and 10.3° for women.) Most subjects moved the body to some position intermediate between the vertical of the tilted room and the true upright. Others succeeded in bringing it to the true upright, regardless of the position of the room, basing their judgments entirely on bodily sensations. Still others, at the opposite extreme, moved the body into alignment with the tilted room, indicating that the visual basis of judgment alone was being used, to the neglect of bodily sensations. This visual mode of orientation is dramatically illustrated in the following result. In Series 3a, where room and chair were tilted to the same side, some subjects reported the body as tilted to the side *opposite* to its actual tilt. With the chair at 22° left and the room at 35° left, for example, such individuals judged themselves to be tilted *right* and requested that they be moved to the *left* to become straight. When the chair was brought to a position of near-alignment with the tilted field, they judged themselves to be upright. This obviously illustrates the use of a purely visual basis for perception of position. Experiments have established that contradictory postural sensations are actually suppressed under the influence of these strong visual impressions, even when large objective tilts of the body are involved. Most of the subjects who judged their body position on such an extreme visual basis proceeded in a confident and untroubled manner. A few, however, experienced great difficulty. When the body was moved into full alignment with the field, so that it looked upright, it began to *feel* tilted. When it was moved back to eliminate this felt tilt, the lack of alignment with the field made it *look* tilted. Prolonged back-and-forth movement usually failed in such cases to locate a position in which the subject could both feel and see himself as upright.

Turning to the results for the rod-and-frame tests, one again finds a very wide range of performances under all the conditions used. In this situation, where an objectively upright rod is surrounded by a tilted frame in the absence of any other visual field, the rod appears, to almost everyone, to be tilted in the opposite direction to the frame. Accordingly, when asked to adjust the rod to the upright, most subjects displaced it from the upright in the direction of the tilt of the frame. The mean amounts by which the rod was displaced in the two series where the body was tilted (Series 1 and 2) were 16.7° and 18.3° for women, and 12.5° and 14.5° for men. In Series 3, where the body was erect and therefore more useful in estimating the upright, the mean amount of displacement of the rod was 12.1° for women and 7.4° for men. In general, then, the tilted frame exerted a marked influence on the perceived position of the rod, especially when the body was tilted. When individual performances are considered, however, it is found that people differed very markedly in the extent to which they were affected by the frame. Some subjects were able to escape the influence of the frame, both with body erect and with body tilted, and succeeded in bringing the rod to within a few degrees of the true upright. The ability to perceive the rod independently of the field, or to deal with the presented visual situation analytically, undoubtedly reflects the effective use of body experiences as a basis for perception. Other subjects—and these constituted the largest group—"went along with" the frame to various extents, bringing the rod to a position that represented some degree of compromise between the vertical of the tilted field and the vertical indicated by the body. For a third group, perception of the rod continued to be based almost exclusively on its relation to the frame. In such cases the tilted frame itself was usually perceived as fully upright, so that straightening the rod became a simple matter of aligning it with the frame. Among subjects whose perception depended upon the frame in this extreme fashion, a very striking phenomenon was observed. At times, in the absence of any actual movement, the subject would suddenly experience a perceptual shift in the axes of the frame, so that its topmost side would be perceived as its left side, for example—that is, parallel to the left wall of the unseen outer room. When perception of the frame showed this kind of lability, alignment of the rod with the shifted axes of course led to exceedingly large errors in estimation of the upright. Surprisingly, these shifts occurred even when the subject's body was upright. When this happened the subject might report that the rod was upright (and therefore presumably aligned with his erect body) when it formed a 62° angle with his body.

In most cases of extreme adherence to the frame, the subject actually perceived it as properly upright, or nearly so, and his judgments were made quickly and confidently. In some other instances, however, adherence to the frame resulted from a stated inability to make use of the body in judging the position of the rod, so that the frame became the only usable standard for judgment. Subjects in these cases reported that they could not resurrect the axes of the unseen outer room, or that space had "taken on the shape of the frame," or that they could not tell which way their bodies were oriented, and so on. Judgments of the rod under these conditions were made with great difficulty and in very troubled fashion.

Subjects whose perception of the rod tended to be based mainly on its relation to the prevailing field sometimes gave very paradoxical performances. To illustrate: A subject would report that the frame, which was at a 28° angle, was tilted, but that the rod, which was actually aligned with it, was perfectly upright. The subject often failed to notice the obvious contradiction in his report. Although this seems to be a rather stupid kind of error, it is clearly due to a particular kind of perception, rather than to any intellectual difficulty. The rod, being within the field of the frame, is perceived entirely in relation to this field. The frame, on the other hand, is perceived on the basis of its relation to the body. There are thus two perceptual systems involved; and the trouble in such cases is that these are kept separated. This separation undoubtedly has adaptive value; for such people the effect of the field upon their perception of the rod is so compelling that any effort to involve the body in the judgment would present too great a difficulty. This was found to be true when the contradiction in their reports was pointed out and they were encouraged to reconsider their judgments. Their subsequent attempts to integrate their strong visual impressions with more remote bodily experiences almost always proved unsuccessful, and they fell back on their earlier procedure of aligning the rod with the frame. The overlooking of so obvious a contradiction in experience, the resistance offered to recognizing it when it was forced on their attention, and the adaptive consequences of operating in this fashion, all suggest that we are dealing here with an instance of repression in perception. Many other examples of similarly "stupid" errors were found in this work, all lending themselves to the same interpretation.

The range of quantitative results in adjustment of the rod to which these differing modes of perception led may be illustrated with the data for Series 3, where the body was erect. One subject, a male, succeeded in adjusting the rod to within an average of less than 1° of the upright in the eight trials of the series. At the opposite extreme another subject, a female, made errors averaging 40.1° in these eight trials (even though she was sitting erect and therefore had the opportunity of simply aligning the rod with her body). On some of the trials this subject experienced perceptual shifts of the frame.

Considering finally the results for the rotating-room test, great variability among subjects is again found, again depending on differences in manner of integrating visual and postural impressions. With the force on the body forming a 43.7° angle with the true upright, most subjects tilted both room and body in a direction opposite to the direction of this force, when required to make them upright. This alteration of the postural determinant of position thus led to a shift in the perceived positions of both body and field, though the shift did not correspond in magnitude to the change in the force.

Examination of individual performances shows that people were affected quite differently in their perception by the change in the force acting on their bodies. In almost all cases subjects felt tilted, to various degrees, while seated in the upright chair during rotation. Many also perceived the upright room as tilted, again to various degrees. When asked to adjust themselves to the upright, some subjects, at one extreme of the performance range, moved themselves to positions approximately at right angles to the force and thus compensated for it in full. These subjects moved the room to an equally marked extent; for them the force experienced by their bodies was the main determinant in perceiving both body and field position. At the opposite extreme were subjects for whom the room continued to appear fully upright throughout rotation and who therefore did not need to move it at all. In this area of the distribution there were even instances in which its alignment with the field caused the body itself to be perceived as fully upright throughout rotation. This manner of perception reflected a complete disregard of the intense lateral tug on the body. It is clear that under the same field conditions and with a force of the same magnitude and direction acting on their bodies, people differ markedly in their perception.

CONSISTENCY, STABILITY AND GENERALITY OF THE INDIVIDUAL'S MODE OF PERCEPTION

Some results will now be presented which indicate that a given mode of perception represents a pervasive and deep-seated characteristic of the individual.

Consistency of Perception

Up to this point the differences in perception found in each part of the three orientation tests have been described. The next question for consideration relates to the *consistency* of a given person's mode of perception in different parts of a given test and in the different tests. To answer this question correlations have been computed[3] among the basic parts of each test and among the three tests. In view of the existence of important sex differences in performance in these tests (to be considered later), results for men and women have been treated separately. The correlations are presented in Table I. For both men and women, the values are sufficiently high to indicate that the manner of a person's orientation toward the upright is fairly consistent, whether the specific task involves perception of the position of a part of a field, of the field as a whole, or of one's own body. More specifically, a tendency to rely mainly on the visual framework or mainly on bodily experiences represents a fairly general characteristic of the individual's orientation.

TABLE I

INTERCORRELATIONS AMONG BASIC PARTS OF EACH ORIENTATION TEST AND AMONG THE THREE TESTS

Test Situation	Men (N=46)	Women (N=45)
1. Tilting-room-tilting-chair test: Room-adj. (Series 1+2) *vs.* chair-adj. (Series 3a+3b)	.60*	.42
2. Rod-and-frame test: Body-tilted (Series 1+2) *vs.* body-erect (Series 3)	.70	.66
3. Rotating-room test: Room-adj. Series *vs.* chair-adj. Series	.58	.62
4. Tilting-room-tilting-chair test *vs.* rod-and-frame test	.64	.52
5. Tilting-room-tilting-chair test *vs.* rotating-room test	−.51**	−.63
6. Rod-and-frame test *vs.* rotating-room test	−.25	−.18

*Values of .38 or higher are significant at the 1 per cent level of confidence, and those of .29 or higher at the 5 per cent level.

**The negative correlations between the rotating-room test and the other two tests occur because, in the former, "going along with" the visual field results in little or no displacement of body and room from the upright, whereas in the latter it results in larger displacements of body, room, and rod.

Stability of Perception

The next question to be considered is whether a given mode of perception is a transient feature of the person or whether it tends to characterize him over an extended period of time. An answer was provided by a study in which a group of subjects was retested in the tilting-room-tilting-chair situation and in the rod-and-frame situation after a period of more than a year. The test-retest correlations for the two situations, respectively, were .85 and .88 for men and .86 and .87 for women. These values are very high, especially for the types of test used, and indicate marked stability in the individual's perception, as far as orientation toward the upright is concerned.

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[3] When it was necessary to obtain a single score for several different parts of a test, standard scores for each part were computed and an average of these taken.

Relation between Performances in Orientation and Nonorientation Perceptual Situations

The correlations presented in Table I show a fairly high degree of consistency in the individual's perception in situations involving orientation toward the upright. Do the perceptual features shown in these orientation situations tend to characterize the individual's perception generally, or do they appear only in this particular type of situation? To answer this question, a study was made of consistency of perception under widely varied conditions. Employed in the study were the three orientation tests described above and in addition a number of nonorientation perceptual tests.[4] One important result will be considered here.

The three orientation tests involve in some of their parts the ability to deal with a given field analytically, or to perceive a part of a field independently of its surroundings. This suggested that it might be profitable to use, as one of the nonorientation perceptual tests, a situation of the kind employed by Gottschaldt,[5] involving the detection of imbedded figures in a complex field. Accordingly a paper-and-pencil test was devised, consisting of a series of 24 complex figures in each of which a simple figure had to be located. These figures were selected from Gottschaldt's original material. To obtain a wide spread of scores, the task was made more difficult by coloring the complex figures so as to "hide" the simple figures in them more effectively. The manner of presentation was to show the subject first the complex colored figure, then a copy of the simple figure that it contained, and finally the complex figure again with instructions to locate the simple figure in it. Simple and complex figures were never shown together, and a new combination of simple and complex figures was used on each trial. The subject's score on each trial was the time taken to locate the simple figure within the complex one. This imbedded-figures test[6] was given to a group of subjects who also received the three orientation tests. The correlations were as follows: between the imbedded-figures test and the tilting-room-tilting-chair test, .61 for men and .51 for women; between the imbedded-figures test and the rod-and-frame test, .64 for men and .21 for women; between the imbedded-figures test and the rotating-room test, −.36 for men and −.35 for women.[7] It is clear that difficulty in locating the simple figure in this test (that is to say, difficulty in escaping the influence of the complex field) tends to be associated with dependence on the surrounding framework in perception of the upright in the three orientation tests. The significance of this finding is most impressively illustrated in the relation found between chair-adjustment results (Series 3a and 3b) in the tilting-room-tilting-chair test and results for the imbedded-figures test. The correlations between these two test situations were .54 for men and .58 for women. These values indicate that people who found it difficult to extract the simple figure from its context also found it difficult to "separate" their own bodies from the tilted field, and to determine their position independently of their relation to the field. It is indeed a very dramatic finding that a simple, neutral visual design should be perceived by a person on the same basis as the position of his own body. Similarity of perception under such very different circumstances provides very effective proof of the presence of consistent and pervasive characteristics in the individual's perception.

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[4] This study was carried out by S. Wapner and the writer.

[5] L. Gottschaldt, "Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren I; Über der Einfluss Einprägung von Figuren auf ihre Sichtbarkeit in umfassenden Konfigurationen," *Psychol. Forsch.*, 1926, **8**, 261-317.

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[6] The materials for this test and the procedures used in its administration are described in detail in a forthcoming paper by the writer: "Individual differences in ease of perception of imbedded figures."

[7] A value of .38 or higher is significant at the 1 per cent level of confidence and a value of .29 or higher at the 5 per cent level of confidence. The basis of the negative correlations with the rotating-room test is the same as that suggested earlier to explain the negative correlations between this test and the tilting-room-tilting-chair and rod-and-frame tests. In the rotating-room test being influenced by the field results in little displacement of room and chair, leading to *low* scores. In the imbedded-figures test being influenced by the field results in an increase in the time required to find the simple figure, leading to *high* scores.

Sex Differences in Perception

Further evidence of the deep-seated character of the observed modes of perception is the finding that men and women differed markedly from each other in the tests used. The differences were always in the same direction—women "going along with" the visual field more than men and responding less to bodily experiences. Thus, in the tilting-room-tilting-chair test, women in general accepted the room as straight at more tilted positions than did men, and, in attempting to make their bodies upright, tilted them farther in the direction of the field. In the rod-and-frame test, they tended to align the rod with the tilted frame to a greater extent than did men, and this occurred both with body erect and with body tilted.

The same kind of sex difference was observed in the imbedded-figures test, where women did more poorly than men, indicating that their perception of the simple figure was more strongly affected by the structure of the surrounding field. It was also noted in studies made of the role of visual factors in the maintenance of upright posture and balance.[8] One series of these experiments evaluated the subject's ability to stand up straight while surrounded by visual fields of varying structure and stability; and another series tested his ability to balance a platform on which he was standing, with similar variations in the field. It was found that instability of the field caused much more disturbance of steadiness and balance in women than in men.

These differences between men and women in susceptibility to influence by the structure of their surroundings are thus pervasive, and represent a difference betwen the sexes of very great importance. The finding of perceptual differences associated with as broad and fundamental a differentiation of people as sex supports the conclusion that the process involved is related in an essential way to the basic psychological organization of the person.

The Developmental Picture

A study of the performance of children of different age levels in our basic orientation tests[9] has revealed significant differences in perception at different stages of psychological development. This finding also indicates a basic relation between the perceptual process represented in these tests and general psychological organization.

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[8] H. A. Witkin and S. Wapner, "The role of visual factors in the maintenance of upright posture" (in press) ; S. Wapner and H. A. Witkin, "The role of visual factors in the maintenance of balance" (in preparation).

[9] This study was carried out by P. Bretnall and the writer.

The results for one test, the rod-and-frame test, will be cited to illustrate the nature of the perceptual differences found. Figure 2 presents these graphically for three groups consisting of boys and girls 8, 10, and 13 years of age, and a group of adult males and females. Curves have been plotted for each of the three series of

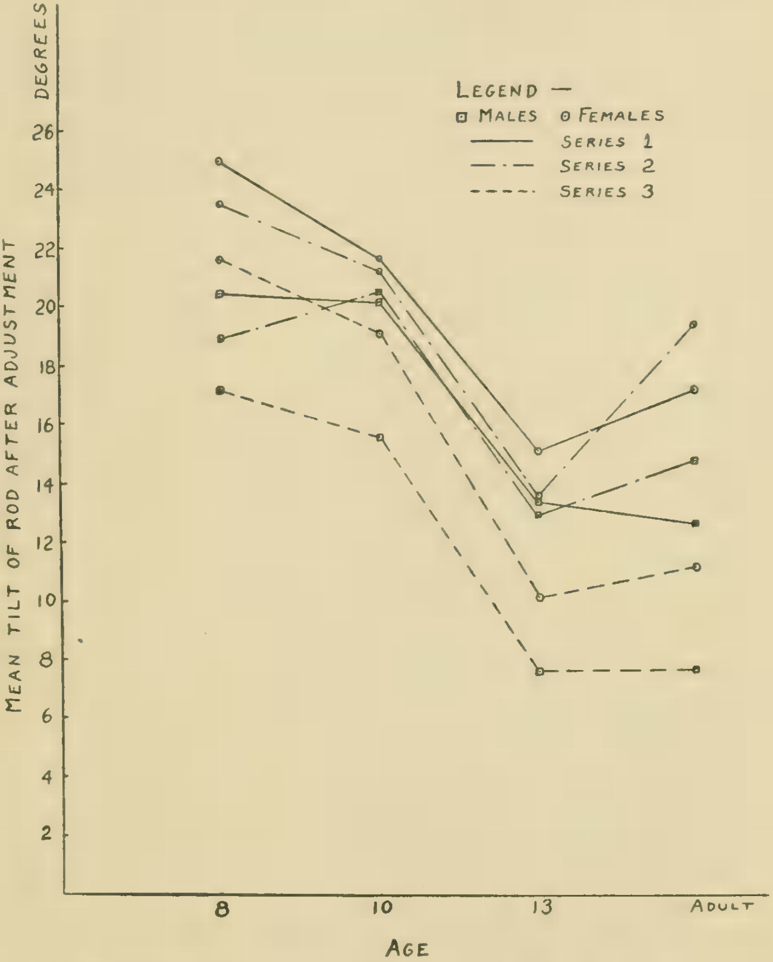

FIG. 2. Results for each series of the rod-and-frame test for different age groups.

the test separately, each point representing the mean amount by which the rod was tilted, when perceived as upright. It should be noted that the intervals between successive age groups are not of equal size and that there is a particularly large gap between the 13-year and adult levels (the latter represents a mean age of about 20 years). This study is still in progress, and perceptual data are now being gathered for additional age groups.

The following main features of the curves found in Figure 2 may be noted:

(1) There is a marked tendency toward better scores (indicating superior ability to deal with the rod independently of the frame) as the age of the subjects increases. There does not, however, seem to be a uniform rate of improvement. Between the 8-year group and the 10-year group there is some improvement, but the difference is rather small. The difference between the 10-year and 13-year groups, on the other hand, is very striking in all three series of the test for both boys and girls. Between the 13-year group and the adult group there is again only a relatively small difference. For women, though not for men, this difference, in all three series, is in the direction of poorer performance. This tendency toward greater adherence to the field is noteworthy, since it represents a reversal of the earlier consistent trend toward increased independence of the field.

(2) A second important feature of the curves relates to the problem of sex differences. For the 8-year group there is a marked difference between boys and girls in all three series, with girls tending to be more dependent on the visual framework. The difference between the sexes diminishes progressively between the 8-year and 13-year levels, so that at the latter age it is quite small. At the adult level, however, the differences become large again.

(3) In earlier studies using the rod-and-frame test (made with adults only) it had been found that subjects resisted the influence of the tilted frame with considerably more success when their bodies were in a normal upright position. This was confirmed by the results of the present experiment, as far as adults and 13-year-olds of both sexes and 10-year-old boys were concerned; these groups made notably better scores in Series 3, where their bodies were upright, than in Series 1 and 2, where they were tilted. It was not true of

10-year-old girls or of 8-year-old boys and girls, however. As Figure 2 shows, performance of these groups in Series 3 was only moderately better than in Series 1 and 2. These findings suggest that for younger children perception of the position of the rod tends to be based more on its relation to the frame, and to be less affected by variation in the position of the body.

These differences between age groups, reflecting developmental changes in perception, are quite striking; and studies directed at establishing their basis are now in progress. These studies will be described in the final section of this paper.

Characteristics of Perceptual Processes as They Relate to the Study of Personal Factors in Perception

The perceptual process involved in orientation toward the upright has a number of features that make it particularly useful for the study of personal determinants of perception. Consideration of these will suggest the kinds of perceptual situations that may be used most profitably in attempts to establish general characteristics of the individual through his perception.

First, adequate orientation toward the upright is of great and continuous importance to the person. Upon it depend not only his ability to make appropriate movements in getting about in his environment but also his ability to recognize the positions of objects with which he must deal. Obviously an incorrect estimate of the upright, with consequent loss of bearings, is much more serious for the person than, for example, an incorrect estimate of the brightness of an object in his environment. It is not surprising, therefore, that subjects in our orientation tests who have difficulty in establishing the position of their bodies or of the field in which they are placed frequently become distressed, and at times even ill—showing symptoms such as sweating, dizziness, and nausea. Disturbances of this kind are never encountered in a brightness-constancy situation, even when the subject has difficulty in equating the disc in shadow with the disc in illumination. This importance of the orientation process to the individual insures his deep involvement in situations that feature it (even under laboratory conditions), and therefore makes it very likely that his performance in such situations will be influenced by general aspects of his psychological make-up.

Second, the process of orientation involves the body directly. Because the body is, as it were, the vehicle of the "self," perceptual processes that depend upon it are more apt to involve important personal characteristics of the individual than perceptual processes that relate mainly neutral objects in the environment. Peculiarities of body structure—such as shortness, stoutness, or weakness—often represent the starting point of personality difficulties. Conversely, general feelings of inadequacy about oneself as a person may result in an image of the body as inadequate, both structurally and functionally. Such a body image may emerge even when it is very much at odds with the actual character of the body as perceived by others. In psychological growth, moreover, there are periods, such as puberty and adolescence, when processes involving the body come to the fore, and the working out of techniques for handling impulses from it becomes especially important. In these and many other ways there exists a two-way relationship between personality organization on the one hand and bodily structure and the body image on the other.

The orientation situations that we have been using, as noted, do evaluate the manner of the individual's perception of his own body, and especially his readiness to experience certain kinds of sensation from it. In view of the importance of the body in personality organization, the results obtained with these situations raise such questions as the following: What does it signify about a person that, when he is tilted as much as 30°, he feels no sensation of tilt because he is aligned with a tilted field and therefore looks straight? What, on the other hand, does it signify about a person that he is able to disregard the influence of the visual field and to respond to even small pressure sensations in determining the position of his body? The answers to these questions are being sought in studies to be described later, in which general characteristics of people whose perception is one or the other of these kinds are being investigated.

A third aspect of the perceptual process represented in these orientation situations is that it is a broad function, depending on the effective integration of a variety of sensory experiences: visual, kinesthetic, cutaneous, visceral, and labyrinthine. This integration represents a higher-level activity. Hence a perceptual process in which it is involved is more likely to be related to other general psychological characteristics, which also depend on higher-level activities, than is a process that is mainly determined by specific sensory effects occurring within a single sense modality. Certainly, personal factors are more likely to find expression in the individual's space orientation than in his color vision, for example. This does not imply that color vision is not influenced by such factors; but it is assuredly less strongly influenced by them.[10]

A fourth characteristic of the three orientation tests is that they involve a conflict between the main sensory experiences ordinarily responsible for perception of the upright. One standard, the postural, proceeds from what the person feels within his own body. The conflicting standard, the visual, is derived from the structure of the outer visual field. Such a conflict is typical of the kind commonly encountered in everyday experience, where one's impulses provide one standard for action, as it were, and the demands and restrictions of the environment provide another.[11] We are now carrying out studies, to be reported later, that seek to determine whether our experimental perceptual situations bring to light the person's characteristic way of handling such conflict experiences and the feelings they engender. Viewed in this more general way, some of the results for the orientation tests acquire added significance, as representing a wide range of individual responses to a well-defined conflict situation.

The above analysis has demonstrated that perceptual processes differ in the opportunity they provide for establishing more general psychological characteristics of the individual; and it has indicated that the process represented in our orientation tests is of the kind in which such characteristics are likely to play a decisive role.

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[10] An example that serves to illustrate the strong influence of personal factors on perception, in situations that involve the person deeply, is the selectivity that occurs in the experiencing of one's environment. In everyone to some extent, and to an extreme degree in people with given kinds of pathology, there may occur a heightened alertness to certain minor events in the environment, or a failure to "notice" certain prominent events. These changes in threshold for given experiences are related to the individual's efforts at adjustment. It is clear that they occur in connection with organized experiences that have a given meaning and a given importance to the person. They are less likely to occur, if at all, in connection with isolated, meaningless events, such as flashes of light or bursts of sound, even when the sensory stimulus is strong. In other words, personal factors are most likely to play a determining role in perception when broad, significant, complexly integrated perceptual experiences are involved.

[11] Here, as in the perceptual situations, the conflict need not be consciously experienced. Furthermore, in both instances, the conflict may lead to the suppression of some of the experiences contributing to it. In the perceptual situations this might help account for the failure to experience sensations of tilt or to take account of the field noted in some of our extreme cases.

Beyond the general characteristics of the orientation process enumerated above, there is an important feature of the test situations themselves that makes them very useful for the study of personal factors in perception. Most people find the task presented in the three orientation tests a "natural" one, and feel entirely comfortable about their final judgments. A decision as to whether an object or one's body is straight may be called for in everyday life. Moreover, once a judgment of straightness has been made, the object really impresses the subject as being straight. This is in contrast to what is found in a brightness-constancy situation, for example, where many subjects are never able to achieve an impression of equality between the disc in shadow and the disc in illumination, so that the decision finally settled upon simply represents the best that can be done under the circumstances. The result obtained in such cases (a given setting of the variable disc) does not directly represent the person's perception. Furthermore, difficulty in reaching a judgment on the basis of immediate impressions often causes the subject to attempt to "figure out" a proper setting for the disc. As a result of this intrusion of cognitive factors, the correspondence between the subject's perception of the discs and his decision in regard to them is further reduced. Because an action representing his perception, rather than a direct account of his perception, is usually obtained from the subject in perceptual studies, it is important that this action be based as directly as possible on his immediate impressions rather than on his interpretations of these impressions. This has been accomplished in our orientation tests.

Average Results and Individual Performances

The emphasis in the results presented above has been on the perception of the *individual.* This interest in the individual was a secondary development in our work. Earlier, our efforts were directed at establishing the roles of such factors as the structure of the field, the magnitude and direction of the force acting on the body, the subject's knowledge about stimulus conditions, etc., in perception of the upright. In this phase of the work the procedure consisted of changing one of these factors and then determining the effect on perception, as reflected in the change in average results for a large group of subjects. This kind of work led to highly significant findings; but it became clear in the course of it that, because of great variation among the individual results constituting these average values, a statement about the nature of perception under a given condition, derived for the group, did not hold true for many individuals in the group. To illustrate: whereas in terms of group results visual impressions were found to be more important than bodily experiences under given circumstances, there were some individuals who disregarded the visual field entirely and based their judgments only on sensations from their bodies. Such findings made it clear that to obtain a full understanding of the process of orientation it was necessary to consider the basis of variability among subjects. In fact, the extent of the observed variation suggested that the problem of why a given individual adheres strongly to the field in his perception of position, and another does not, is at least as important as the problem of the general effect of a given field condition on perception. This transition in our work toward a concern with individual performances was one that is not unusual in psychological studies; and it was assuredly a desirable one, first from the standpoint of the psychologist's fundamental interest in the individual and secondly from the standpoint of the great uniqueness of the individual, psychologically considered, as compared with the basic unit of study found in other sciences.

The relation between group results and individual results presents an important problem for psychological research generally, and the nature of this relation may be seen very clearly in the field of perception. Average data for a group of subjects may establish that a situation of given structure has a given effect on perception. The results are not of a single kind, however, but comprise a range whose limits are determined by the structure of the situation. If the structure is altered, the range as a whole will shift, or its boundaries will be contracted or extended. The fact that a field of stated structure leads to a range of possible results rather than a single result reflects the role of personal factors, unique to the individual, in determining the outcome. Thus, the *full* task of the psychologist consists not only of establishing the average results and the range of results arising from a field of given structure but also of determining the factors within individuals responsible for their location at given points in this range. Only if both steps are taken will it be possible to arrive at fully comprehensive laws of perception.

It is necessary to point out that an emphasis upon personal determinants of perception need not lead to an extreme subjectivist interpretation (i.e., "People see what they want to see."). A belief that personal factors should be taken into account in the analysis of perception does not imply that his manner of perceiving things is simply the product of the whim of the individual. That perception is bound to the structure of the real world in which it takes place is proved by the fact that the variations caused by the operation of personal determinants are confined within the limits of a determinable range. When the perception of an individual falls outside of this range (as in the extreme case of a person who hears voices in the absence of any sound in the environment), there is good reason to suspect some pathological condition.

Finally, because a given field determines a range of possibilities in perception, it is important in perceptual studies to employ an adequate number of subjects. Studies performed in the tradition of orthodox field theory have frequently used just a few cases, on the premise that the structure of the field determines perception in essentially the same way for everyone. Investigations conducted in this way not only make it impossible to obtain information about differences in perception, but may even—by failing to obtain the full range of results possible in the situation—lead to incorrect average results and therefore to invalid generalizations.

The Study of Perception-Personality Relationships

Since the beginning of 1947 the main work of our laboratory has been directed at establishing the personality patterns associated with the different modes of perception described above. This work is still in progress, so that it is not yet possible to report results; but the procedures used may be described. Essentially, there have been three areas of investigation represented in the study.[12]

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[12] Collaborating in one or more of these areas have been P. Bretnall, M. Hertzman, K. Machover, S. Wapner, and the writer.

Perception-Personality Relationships in a Normal Population

The basic orientation tests were given to a large group of Brooklyn College students, and distributions were established from men and women separately. On the basis of these distributions, a representative group of each sex was selected for intensive study. Each subject received an additional series of perceptual tests as well as an intensive personality analysis in which the following techniques were used: an individually administered Rorschach Test, a Thematic Apperception Test, a clinical interview, a Draw-A-Person Test, a word-association test, a sentence-completion test, and an autobiography.

Perception-Personality Relationships in an Abnormal Population

The results for the Brooklyn College group suggested the desirability of obtaining more extreme cases for some of our diagnostic categories than could be found in the Brooklyn College population. They also indicated the need for investigating certain diagnostic categories not represented at all in the normal population. Accordingly an abbreviated series of our perceptual tests was set up at the Psychiatric Division of the Kings County Hospital in Brooklyn and administered to a group of patients selected to supplement our data in the desired ways. These patients also received an intensive personality analysis corresponding to that given the Brooklyn College group.

Perception-Personality Relationships in Children

An earlier section presented some results of a study of developmental changes in perception. In addition to establishing perceptual "norms" for different age levels, this study is also investigating perception-personality relations at each age. The objectives are, first, to determine the basis of the observed differences in perception at different ages, and, second, to establish the origin of the variation observed within each age group.

Groups of boys and girls of different age levels—8 years, 10 years, and 13 years—were given an abbreviated series of our perceptual tests. The 8-year level was chosen as the earliest one to work with, in view of the difficulty found in applying our rather complex perceptual techniques to younger children.

After perceptual results for boys and girls of these age levels were obtained, smaller representative groups of each sex were chosen for intensive personality study. Used for this purpose were an individually administered Rorschach Test, a Thematic Apperception Test, a Draw-A-Person Test, and a Play Situation. To evaluate the physical maturity of the children, skeletal age was determined through an analysis of wrist bone development as established by X rays.

During the coming year additional age groups will be studied, using the procedures described.

The rich and extensive material provided by these studies, both on the perceptual side and on the personality side, should make it possible to establish the relation between perception and personality in a comprehensive fashion.

AUTONOMIC DISCRIMINATION WITHOUT AWARENESS: AN INTERIM REPORT

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This paper is an interim report of an experiment which confirms the notion that subjects give discriminatory galvanic skin responses to visual stimuli presented at tachistoscopic speeds too brief for correct verbal report. The implied perceptual process is termed by the authors *subception* (verb form—to *subceive*).

Research and theorizing in the area of perceptual dynamics has, to a great extent, concerned itself with observations of differential recognition of various stimuli as a function of the organism's needs. One common experimental approach has been to present to subjects words or pictures of varying motivational value at exposure speeds ranging from impossible to easy in recognizability. Measurements are made of the exposures at which recognition occurs. Differences in recognition thresholds are then correlated with the differences in the motivational import of the stimuli. It has been reported frequently in such research that stimuli of different need significance may have different recognition thresholds (**1**, **4**, **6**).

Attempts to interpret this sort of phenomena have led workers into the difficult position of having to postulate some process of discrimination occurring prior to the ability to report correct recognition. Such a concept as perceptual selectivity falls into this category (**1**, **6**). Simply stated, if the observation that a subject is capable of recognizing the word *sacred* at a faster exposure than the word *income* is attributed to the differential need value of the words, it seems necessary to postulate that the subject is somehow identifying the significance of the two words prior to his reported recognition of them. The major purpose of the present research is to determine whether such a mechanism might operate in visual situations comparable to those employed in other "need in perception" research. Previous experiments dealing with "sub-liminal" perception (to be reviewed in the complete report of this research at a later date) have not employed experimental techniques sufficiently comparable. For example, previous researches have often depended upon ambiguous instructions to the subject, or did not have an independent measure of perceptual behavior, and often used the subject's subjective certainty or lack of certainty to evaluate the "sub-liminal nature" of the stimulus.

In addition, the experiments reporting on this subject have rarely concerned themselves with the possibility that apparent differences in perceptual accuracy reflected nothing more than differences in response preferences. For example, the subject who reports recognition of the word *sacred* at faster exposures than the word *income* might not be manifesting a real perceptual difference. When forced to give prerecognition guesses, this subject might merely be more familiar with the word *sacred,* and, therefore, be more apt to respond with it sooner, or with less available cues, than with the word *income.* This research is concerned also with measuring differences in perceptual accuracy as a function of need, while controlling for response preferences. As you will note later in this report, the tachistoscopic procedures we used offer considerable change over earlier work of this sort.

PROCEDURE

GSR's of the subjects were recorded by means of an A.C. bridge apparatus.[1] Dry silver recording electrodes, ¼" square, were attached to the right forefinger and right middle-finger tip of the subject. GSR's were read directly as peak deflections on a Ballantine Electronic Voltmeter. These readings were converted to actual impedance changes by substituting a variable resistance box in the circuit in place of the subject, and matching the voltmeter readings, obtained when the subject was in the circuit, with the phase angle of the bridge set at zero. It was possible to read pure impedance change because the A.C. bridge was designed so that the subject's apparent capacitance change could be balanced-out during the experiment, by operating a dial that kept a Lissajous figure closed when the phase angle of the subject and the bridge were exactly equated.

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[1] The authors are indebted to Thomas E. Arnold, at present in the Department of Medicine, Johns Hopkins Hospital, for the design and development of the GSR apparatus used in this research.

This impedance change (ohms) was transformed mathematically into admittance change (mhos) in keeping with physiological (2) and statistical (3) suggestions as to the most meaningful unit of measure for the GSR.

Stimulus words were presented on a beaded screen seven feet from the subject by means of a projection tachistoscope. This arrangement allowed for a variation of exposure speeds from 1/150 sec. to 1 sec. The illumination and range of exposure speeds varied from subject to subject depending on his perceptual performance, and, once established, were held constant for each subject. Five different exposure speeds were selected for each subject such that the slowest speed resulted in near 100 per cent accuracy of recognition of all syllables. In all cases, the fastest exposure speed resulted in accuracy of recognition which did not differ significantly from chance.

Five-letter nonsense syllables were used as the stimuli, to minimize as far as possible differences in familiarity. Ten syllables were used, and they were as follows: YILIM, ZIFIL, GAHIW, GEXAX, JEJIC, JIVID, YUVUF, ZEWUH, VAVUK, VECYD.

The procedure for each subject can be divided into three periods:

Equation period. After practice at recognition each subject was presented the ten nonsense syllables tachistoscopically and randomized for both order and exposure speeds. Even at the speeds where accurate recognition was impossible, each subject was required to make a choice verbally from the 10 syllables that he knew made up the stimulus material being presented to him. To aid in this choice, each subject was given, in succession, 10 different lists of the 10 syllables. Each list was in a different order to preclude list-order preference on the part of the subject. After these 100 presentations, the 10 syllables were divided into two groups of 5 each, being equated for both the number of times the 5 syllables were used in the entire 100 responses, and the number of times the 5 syllables were correctly recognized. In all cases it was possible to make this two group division with a high degree of equality. Also, in this phase of the experiment, the exposure speed at which near 100 per cent accuracy of recognition occurred was noted for use in the third period of the experiment.

Conditioning period. In this part of the experiment we employed a one-second exposure speed for all syllable presentations. The GSR was conditioned to the five experimental syllables, using electric shock as the unconditioned stimulus. Partial reinforcement was employed, with one third of all presentations of the five experimental syllables being shocked in random order. During this conditioning period, each of the 10 syllables was presented an equal number of times to prevent unequal familiarity with the syllables. This procedure was continued until consistent conditioned responses to the five experimental syllables were established. Subjects were instructed not to report during the conditioning trials but merely to identify to themselves the syllable presented to them. They were instructed at the beginning of the conditioning trials that when they were shocked it would be 2/3 seconds following some presentations of the experimental syllables. They were further instructed that they would never be shocked after presentations of the five control syllables.

Final test period. The random syllable presentation procedure used in the Equation Period was repeated here. The exposure speeds were set so that the subject had near 100 per cent recognition at the slowest of the five exposure speeds used. This time, however, GSR's were recorded during the time between the tachistoscopic flash and the subject's verbal report. To minimize the effect of the subject's verbal report on the GSR, the subject was instructed to delay his report until signaled. This signal was not given until the GSR was recorded for each syllable presentation.

During this part of the experiment no shock was paired with any of the syllables. However, because of instruction and prior experience with partial reinforcement, the subject's expectancy of being shocked, and consequently the conditioned response, was maintained throughout the 100 syllable presentations of the final test period.

The following data were recorded during the Final Test Period:

1. The syllable flashed.
2. The exposure speed of the syllable.
3. The subject's verbal choice from the 10 possible syllables.
4. The subject's GSR for each stimulus presentation.

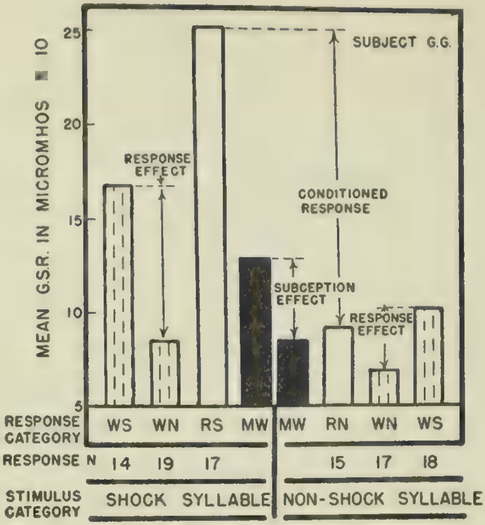
RESULTS

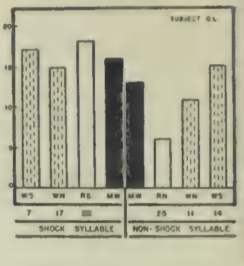
Not all the possible analyses of the data at hand are being presented in this interim report, but will be published later when a larger N has been accumulated. There is one striking finding, illustrated in the bar graphs, which is the main result reported here.

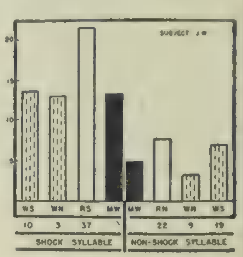
These bar graphs show the GSR data obtained in the final test period. The GSR's are put in columns in these graphs according to the category of syllable presented (syllables associated with shock and syllables not associated with shock), and the nature of the subject's verbal report: the symbol 'W' means that the report was wrong; 'R,' that it was right; 'S,' that the subject reported one of the 5 syllables previously associated with shock; 'N,' that the subject reported one of the 5 syllables never associated with shock. 'Response N' refers to the number of separate GSR's contributing to the mean GSR for each category. *The two 'MW' (mean wrong) categories are the important GSR measures to note.* These represent the average GSR of the 'WS' and 'WN' categories for each of the two stimulus groups. Therefore, these 'MW' columns represent the average GSR (equated for both S and N responses) when the subject was not able to perceive correctly (i.e., report) the presented syllable.

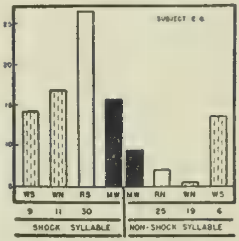
You will notice that in every subject the MW column is larger when the stimulus presented was a 'shock syllable'—and you will recall that in this 'MW' category the subject was not able to perceive the syllable correctly. This consistent 'MW' difference is summarized as the combined data for all subjects in the last bar graph. Student's t for this difference is 4.91, which is significant below the 1 per cent level of confidence for 4 degrees of freedom. Further subjects are in the process of being run, but there does not seem to be any great doubt that this is a true difference.

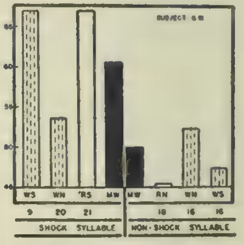
In an experimental procedure necessarily as involved as this one, it might well be suspected that the 'MW' difference reported above could be the result of some artifact. This possibility is considered briefly in the conclusions, but at this point it is encouraging to note that the further analysis of this 'MW' difference (cf. Table I) for different exposure speeds, classified in terms of the subject's accuracy of recognition at the various speeds, demonstrates the difference to

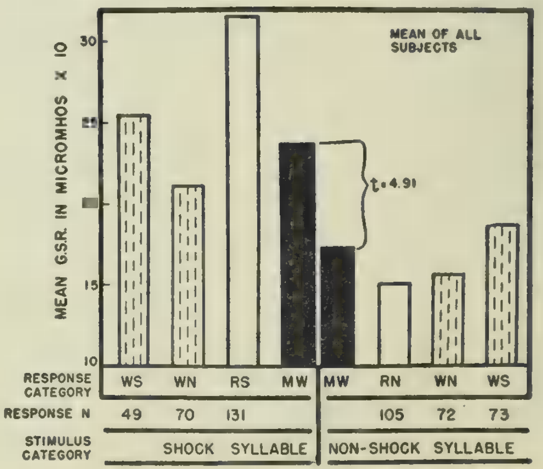

MEAN G.S.R. IN MICROMHOS × 10
25
20
15
10
5
SUBJECT G.G.
RESPONSE EFFECT
CONDITIONED RESPONSE
SUBCEPTION EFFECT
RESPONSE EFFECT
RESPONSE CATEGORY
WS
WN
RS
MW
MW
RN
WN
WS
RESPONSE N
14
19
17
15
17
18
STIMULUS CATEGORY
SHOCK SYLLABLE
NON-SHOCK SYLLABLE


SUBJECT O.L.
20
15
10
5
0
WS
WN
RS
MW
MW
RN
WN
WS
7
17
25
11
14
SHOCK SYLLABLE
NON-SHOCK SYLLABLE


SUBJECT J.W.
20
15
10
5
WS
WN
RS
MW
MW
RN
WN
WS
10
3
37
22
9
19
SHOCK SYLLABLE
NON-SHOCK SYLLABLE


SUBJECT E.G.
25
20
15
10
0
WS
WN
RS
MW
MW
RN
WN
WS
9
11
30
25
19
6
SHOCK SYLLABLE
NON-SHOCK SYLLABLE


SUBJECT G.M.
65
60
55
50
45
WS
WN
RS
MW
MW
RN
WN
WS
9
20
21
18
16
16
SHOCK SYLLABLE
NON-SHOCK SYLLABLE


MEAN G.S.R. IN MICROMHOS × 10
30
25
15
10
MEAN OF ALL SUBJECTS
t = 4.91
RESPONSE CATEGORY
WS
WN
RS
MW
MW
RN
WN
WS
RESPONSE N
49
70
131
105
72
73
STIMULUS CATEGORY
SHOCK SYLLABLE
NON-SHOCK SYLLABLE

be present and in the same direction at all levels of accuracy taken individually. Further, it will be noted that the 'MW' difference as a function of the subject's accuracy reveals a monotonic function. As the subject's accuracy increases, the 'MW' difference tends to become larger. It may be that the drop in the 'MW' difference in the '60 per cent to 79 per cent interval' represents a true drop as recognition approaches 100 per cent. On the other hand, the drop may mirror the instability of this point where there are necessarily fewer W responses on which to base the 'MW' evaluation.

TABLE I

Analysis for 'MW' Difference for Different Exposure Speeds with Speeds Classified According to the Subjects' Accuracy of Recognition

Percent Accuracy of Recognition	0%—19%	20%—39%	40%—59%	60%—79%
(a) 'MW' for presentation of 'shock' syllables............	18.0	26.0	23.9	65.5
(b) 'MW' for presentation of 'nonshock' syllables.........	15.6	20.3	7.3	52.4
(a) minus (b) ('MW' difference).................	2.4	5.7	16.6	13.1

There is one other interesting aspect of our results which will be commented on briefly, although the actual data will not be presented until our complete report. When the accuracy of recognition for the shock and nonshock groups of syllables at each of the five speeds was equated for the frequency with which the syllables were used (perceptual accuracy corrected for response probability), the syllables associated with shock in the conditioning period turned out to be correctly recognized more frequently than the nonshock syllables. This was true in four of the five subjects. Moreover, as you can see in the bar graph summarizing the data for all subjects, the 'N' syllables were used more frequently as responses when the verbal report was wrong (NW). Despite this there are more correct 'S' responses (RS) than correct 'N' responses (RN). Since the two groups of syllables were equated for accuracy and response frequency during the preliminary Equation Period, and presented to the subject an equal number of times during the Conditioning Period, this difference in accuracy is presumably due to the pairing of the five experimental syllables with shock. With the present number of subjects, this trend is not statistically significant, and evaluation of it must await the completion of the present research.

CONCLUSIONS

There is one clear conclusion that we should like to present. It is that even when a subject is unable to report a visual discrimination verbally (i.e., he reports incorrectly when forced to make a choice), he is still able to make a stimulus discrimination at some level below that required for conscious recognition. This is indicated by the consistent discriminatory Galvanic Skin Responses in the absence of correct verbal report. We suggest that this level of perceptual activity be called *subception*. Our research is not the first to suggest evidence for this kind of a perceptual process. As a process it is implicit in certain clinical concepts. It is clearly a postulate in much of the theorizing in the "need in perception" field. There would seem to be practical value in having a term to cover this aspect of perceptual activity.

Alternative explanations of our data have been suggested to us, but the nature of the experimental findings refute them.

1. Was the subject able to detect minimal cues that 'tipped him off' that a shock-syllable was being presented even though the cues were not sufficient to permit precise report of the syllable in question? It is clear that this kind of explanation does not apply. If this were the case, the subjects would have been able to call correctly the shock *vs.* nonshock nature of the syllable. There is no trend in the data to suggest this. In fact, as you can see in the bar graphs, 'N' responses were more commonly given to 'S' stimuli, and 'S' responses more frequently reported when an 'N' stimulus was flashed.

2. Are the GSR's actually responses to the subject's own report of what he thought the syllable was? As is clear from the account of our procedure, all possible controls for this difficulty were employed. Despite these controls, it is clear from the bar graphs that to some degree this 'response effect' was present, although it was variable in extent and direction. Our use of the 'MW' category,

however, equates for differential 'response effect' so that it does not influence the 'MW' difference that is evidence of *subception.*

3. Did the degree of uncertainty of the subjects at subthreshold exposure speeds lead to increased autonomic activity? This would be expected to affect both shock and nonshock stimulus categories alike. Moreover, comparison of the GSR's for wrong and right responses to syllables never associated with shock indicates no such 'uncertainty effect.'

Despite the highly significant t value substantiating the *subception* effect, the small N employed so far in this study indicates some degree of caution until our procedure can be checked out on a larger group of subjects. If appearances to date are not deceiving, however, our data show that some form of prerecognition discrimination is possible and actually occurs in certain visual perceptual situations.

SUMMARY

1. GSR evidence is presented to indicate that at tachistoscopic exposure speeds too rapid for conscious discrimination (as measured by the subject's inability to report what stimulus was presented), the subject is still capable of responding in a discriminatory way.

2. It is suggested that the level of perceptual activity indicated by this finding be called *subception.*

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TOWARDS A REFORMULATION OF THE PERCEPTION-MOTIVATION DICHOTOMY[1]

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The Separatist Approach

A considerable number of studies have appeared during the last decade dealing with the so-called relationship between motivation and perception. Most of these have been successful in demonstrating the "effects of needs upon perception"; therefore the empirical phenomena as such seem reasonably established. The theoretical bases of such investigations, however, are seldom stated in more than a vague and equivocal form, and, explicitly or implicitly, devolve upon the view that motivational forces are distinct, separate, and altogether different from those that determine the nature of the perceptual field. "Value and Need as Organizing Factors in Perception" (2), "The Relation of the Intensity of a Need to the Amount of Perceptual Distortion" (6)—even the titles indicate a "separatist" theory of motivation.

Of course, it has been recognized that the cleavage between the cognitive and conative phenomena is far from sharp, and an attempted resolution has been made by the introduction of a concept of "interaction" between motivational and cognitive processes. Since this, however, is conceived as an interaction between primarily split and separate processes, which interact only *secondarily*, it can only be considered an unsuccessful attempt to patch up what remains fundamentally a separatist theory of psychology. In maintaining the traditional separation of these psychological processes, one is faced not only with the difficulties of perception psychology alone, or of motivation psychology alone, but with the problems of one multiplied by the problems of the other. It is difficult to conceptualize the common ground upon which "perception" and "motivation" can meet and interact. So long as they are couched in different dimensions, the dualism which had postulated the pineal gland as the point

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[1] The writers wish to express their indebtedness to Dr. David Krech and to Dr. Edward C. Tolman for their helpful advice and criticism.

of interaction between soul and body is, at best, but lightly concealed. In discussing the effects of needs upon perception, such a theory can make little more than vague predictions as to the nature and direction of the effects obtained; this weakness in predictive power, moreover, is not improved by a marked tendency towards the use of *ad hoc* hypotheses in the explanation of experimental results.

The ease with which this theoretical approach lends itself to such hypothesis formation is a danger signal. No less than four different hypotheses are necessary to account for the results in a pioneer experiment by Levine, Chein, and Murphy (6), but there is no doubt that a fifth or sixth could be constructed just as readily if the data should require it. In that experiment the subjects looked at "ambiguous" figures through a ground-glass screen while hungry. A reasonable hypothesis might be that the number of food interpretations would vary monotonically with the strength of the drive. Unfortunately, the data did not comply with this simple hypothesis: the number of food interpretations fell sharply as the period of fasting rose to six or nine hours. To explain this discrepancy the experimenters postulate that now the subject is too hungry to stay in the situation. He wants real food rather than pictures, and therefore rejects the experimental situation. Moreover, since the drop occurs not only as a result of an increased number of "rejections" (where the subject is unable to supply an interpretation), but also because of a decreased percentage of food interpretations among all the interpretations given, one must further assume that the subject not only wants to get out of the situation but that in addition he wants to avoid all thoughts of food: ideas of food are warded off and converted into neutral percepts as a defense against an otherwise frustrating situation.

If one postulates that needs affect perception, there is always an extra need that can be called upon if the experiment should yield more complicated results.

What seems so objectionable, however, is not so much the complexity of the theoretical formulation—for, after all, we have no right to expect the organism to behave as simply and uncomplicatedly as our aesthetic preferences might lead us to wish—but rather the fact that there is no direction in the separatist theory which would give any prior hint or clue as to the nature of the required hypotheses. Moreover, the separatist view even has difficulty in retaining the gains made within the fields of motivation and perception. By its very patchwork nature it is in large part unable to make use of the existing laws and findings within, for example, the field of perception proper. This poses an especially strange spectacle in studies presumably conducted under a Gestalt orientation, where too often the field forces are not only ignored in the avowedly "organizational" discussion of needs, but are not even applied to the perceptual processes with which the needs are supposed to interact. Thus, Bruner and Goodman (2) compare size estimates of coins with those of "neutral" discs whose texture and articulation is even more different from that of the coins than are the coins from each other. The fact that poor children overestimate the size of coins more than do rich children strongly suggests some effect of "value" and brings an extremely important variable into the psychological laboratory. Nevertheless their experimental results offer only presumptive evidence in favor of their hypothesis in view of the comparative neglect of "autochthonous" forces of organization.[2]

The compromise view thus finds itself in an almost more difficult position than did the completely insular one it had tried to replace; the postulation of an "interaction" effect between essentially separate processes has removed none of the problems caused by their separation and may even be said to have added a few. In a way this situation seems somewhat analogous to the history of the Ehrenfels concept of *"Gestaltqualität,"* a theoretical hybrid that combined both sensations and form elements and found itself under fire from both sides—later to be supplanted by the more thoroughgoing, radical revision of the Gestalt school proper, which completely removed the dichotomy between sensation and perception by denying the existence of the former.

A theoretical alternative is clearly in order.

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[2] Examples of experiments which have attempted to consider these "autochthonous" factors as an important variable in the experimental situation, are Ansbacher (1), who controlled the configurational differences between "valued" and "neutral" objects, and Torrey (8), who emphasized the effect of configurational attributes of various tasks on their Zeigarnik ratio. But, on the other hand, neither of these experiments gave much attention to social or "personality" variables, and this limits the generality of their conclusions from another side.

NONSEPARATIST ALTERNATIVES

Two alternative possibilities suggest themselves immediately:

(1) Perceptual processes can be subsumed under motivational ones—in the sense that they can be described and predicted by motivational laws.

(2) Motivational processes can be subsumed under perceptual ones—in the sense that they can be described and predicted by perceptual laws.

In each of the alternatives, both motivational and perceptual processes are considered as manifestations of some underlying system—except that this underlying force system would very much resemble need theory in the first alternative, and perceptual theory in the second.

The purpose of the present paper is to explore some of the possibilities arising from the acceptance of the second alternative, which would subsume motivational phenomena under perceptual laws. Certainly the laws of perceptual organization are more precise, well defined, and on more solid empirical foundations than whatever laws of needs exist at present. The present state of knowledge in the so-called sphere of motivation is extremely vague and unformalized. As a consequence, the initially postulated needs can be multiplied almost endlessly in the course of the explanation of any experimental situation, and their addition and manipulation can proceed altogether unhampered by any theoretical restrictions. This leads almost inevitably to the ultimate dilution of any explanatory schema to a pseudo-clinical description of little or no generality. These considerations alone suggest some of the drawbacks in a motivation-based nonseparatism. In contrast, the hypothetical constructs employed in the sphere of perception, although perhaps phenomenologically further from real life, present a more formal structure and a far more objectively analyzable set of relationships than can be found in present motivational systems.[3]

However, a more basic difficulty in a motivation-based nonseparatism is caused by the implicit assumption of a Pure Ego, in much the same sense as was deplored by William James. Need-oriented theoretical frameworks generally imply a "perceiver" who operates in a rather vague and equivocal relationship to the causal structure of the perceptual situation. Thus, in a recent experiment by Postman, Bruner, and McGinnies (7), failure to recognize a given word is interpreted as due to its being "blocked by perceptual defense mechanisms." As the authors themselves point out, this kind of interpretation is almost forced to presuppose that the sentence was first perceived—before it wasn't! In effect, this implies the existence of a perceiver, a Pure Ego, who initially cognized the percept, then rejected it. This widespread mode of interpretation (particularly strong among the theoreticians of projective techniques) probably represents an attempt to bring some principle of order into the disjointed and largely unconnected atoms of need theory. Faced with the behavior and productions of an organism which is to a high degree ordered and integrated, the need theorist can unify his battalion of needs only by placing them at the disposal of a small simulacrum of the organism, almost an organism in miniature, placed somewhere within the "personality."

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[3] Krech (**5**, p. 72) raises the suggestive point that closeness to "real life" and phenomenology are not the touchstones of scientific virtue; their value is as starting points, not as end-results: "Our concepts must be adequate enough to permit us to encompass all experience and all behavior, but our concepts cannot be as pluralistic as our experiences and behavior patterns."

This conceptualization of a Pure Ego flirts quite dangerously with vitalism and would seem to reduce psychology to a homunculus situation, in which all of the important problems are eventually relegated to a "higher" level—perhaps ending in an infinite regress of the psychologies of a never-ending, concentric series of homunculi.

While this concept seems very difficult to eliminate from need theory, perception psychology, on the other hand, is under no compulsion to accept it. Although it is true that the phenomenally given and vitally important fact of the "me" cannot be overlooked by any psychology, a perception-oriented nonseparatism can handle this problem by considering the ego as part of the perceptual field. The ego, according to this view, is a perceived "object" or relation between objects within the psychophysical field, rather than a perceiver in the Pure Ego sense, abstracted from the perceptual field and regarding its percepts from the outside.

Such considerations seem to throw the balance in favor of a perception-based nonseparatism as opposed to a theoretical framework stemming from need theory. It must be admitted, however, that there is clearly a third alternative which so far has been ignored:

the method of considering both motivation and perception as manifestations of an underlying common system of variables, which in itself is neither perceptual nor motivational in character, but of which the cognitive and conative phenomena are more or less arbitrarily abstracted part-processes. This is the type of approach advocated by Krech (5), whose "Dynamic systems" represent an attempt to postulate exactly such an underlying force system. From such a point of view, the bias of the present paper might perhaps be dubbed a "perceptual Imperialism." It is difficult to see, however, how even the most "revolutionary" formulation—to use Krech's phrase—can escape the taint of some sort of imperialism at present. For, while the assumption of an underlying force-system seems acceptable as an over-all orienting principle, specific laws and properties will have to be ascribed to this in the course of actual theory construction, and it seems reasonable to suppose that these will be drawn from laws and properties obtaining in those of its part-processes thus far studied. Although both cognitive and conative phenomena are admittedly abstracted from a more fundamental underlying system, it is suggested that the relationships holding within this system are more adequately described by laws formulable within the framework of perception psychology than by those of need theory.

THE "PERCEPTUAL" ALTERNATIVE

One does not have to go far to find a base of operations, for the Gestalt laws of perceptual organization immediately suggest a theoretical framework, built upon the fundamental law of pregnanz. This law implies far more than the closure of incomplete circles or the good continuation of figures: it is essentially the application of thermodynamic principles to perceptual processes, a statement that all perceptual processes seek to maintain equilibrium (4). Disequilibrium is constantly created by the effects of stimulation producing changes in the isomorphic field, and just as constantly the field reacts in the direction of restoring equilibrium. Some of the more obvious of these equilibration processes are revealed by the very closure effects and completion phenomena which the law of pregnanz was first designed to encompass. Square after-images tend to be rounded off, incomplete circles will be perceived as closed under sufficiently low illuminations or short exposures, and completion phenomena have been demonstrated in the blind spot and in the blind half of the visual field of hemianoptic subjects. In all of these cases the perceptual system "does" something—it equilibrates.

In the examples given above, the equilibration involved might conceivably take place without overt muscular concomitants, but there are many instances in which perceptual equilibration cannot take place by such "internal," "non-motoric" changes alone. In these cases the stimulating conditions themselves must be altered to achieve equilibrium, and the alterations of the stimulating conditions are brought about by the motor aspect of the organism. Perhaps the best illustrations are provided by eye movements: A stimulus object is placed against a homogeneous ground, somewhat off the center of the field. Immediately the eyeballs move so that the retinal image is placed upon the fovea. But while this change occurs quite naturally, quite "unconsciously," it is very hard to explain as a reflex. Disequilibrium created by the peripheral position of the stimulus is corrected for by changes in the oculomotor system, which alter the stimulating conditions, and thereby tend to restore equilibrium. Accommodation and convergence phenomena have similarly been shown capable of being dealt with in this fashion—equilibration processes involving the organism's "motoric" (3, pp. 311-317). However, there is no reason to restrict this analysis to eye movements alone—in principle, the same analysis might be applied to head movements related to sight or audition, postural adjustments, and so forth.

But is all this so very different from the case of a child running away from a loud noise or reaching for a bar of chocolate? A disequilibrium has been created, and as in the examples of the eye movements, perceptual equilibration proceeds in the only way possible: the stimulating conditions are changed by the participation of the organism's motor processes. All, or at least most, of these "goal situations" require but one more complication in the theoretical postulation. The disequilibria so far mentioned have occurred between isomorphic representations of objects or stimulus configurations in the perceptual field. If one now assumes that the organism—as a cognized object rather than as a perceiver in the "pure Ego" sense—may be likewise represented in the isomorphic field, disequilibria occurring between it (the "cognized" Ego) and other bounded regions ("goal objects") can be considered similarly. At least to the extent that the thing-constancies obtain (i.e., to the extent that the behavioral environment bears a close relationship to the geographical environment), the resolution of such disequilibria can be achieved only by actual changes of the relationships between organism and goal-object in the objective, geographical environment. Thus, the organism moves about in the environment until perceptual equilibration is brought about. Within the Gestalt theoretical framework here adopted, this type of equilibration corresponds to what is generally called goal-directed behavior. From such a point of view, the "motivation" is neither superimposed from above nor injected in from below, but is an attribute of the total field situation.

In all of the foregoing it has been assumed that motor activity takes place to bring the *perceptual* field to equilibrium: where the balance cannot be restored by the isomorphic system as it stands, a motor adjustment occurs, which, by changing the stimulating conditions affecting the organism, creates, or tends to create, the requirements for perceptual equilibrium.

Of course it is very difficult, if at all possible, to abstract the operations of the motoric from those of the perceptual system. The actions of the motoric are hard to explain from a consideration of the motoric itself, nor can the changes in the perceptual field be easily analyzed in terms of perceptual processes only. The two systems are probably best understood as part-processes of a larger system, since they do not increase in entropy independently, but rather tend to equilibrium as a coupled system. In the more complex situations involving locomotion through the environment, it becomes even more evident that the static conditions under which most of our perceptual laws were presumably discovered can be considered only as a first approximation. For, in general, perceptual space and the objects within it can be based only upon the changing pattern received by a *moving* organism, while the movement of the organism must in turn depend upon its perception of space. The experimental conditions of the psychological laboratory, with its chin- and head-rests, and its constant fixation points, are seldom if ever encountered in the ordinary life of an organism. Nevertheless, there seems to be some reason for believing that the "radix" of the perceptual-motor field is largely perceptual in nature. The equilibrium of the oculomotor system, for example, is, as Koffka points out (3, p. 316) determined primarily in terms of the equilibrium within the perceptual subsystem; in fact, this is always accomplished at the expense of some continual disequilibrium within the motor system. The phenomenologically evident constancy and continuity of our perceived environment further implement this belief. Again it appears more fruitful to accept the established laws of perception as a first approximation towards the laws of the perceptual-motor system.

This, then, would constitute one approach to a theoretical framework for a nonseparatist theory of motivation. In essence it is hardly more than a restatement of the Gestalt theoretical position, and in any case seems to derive completely from the fundamental postulates of the Gestalt theory of perceptual organization. Such a theory of motivation would consider all motivational phenomena as manifestations of equilibratory processes occurring in the psychophysical field. From this point of view, the formally simplest motivational phenomenon might be—not the rat running towards food, not the infant establishing object-relations with its mother—but the much more prosaic case of an incomplete circle tending to close.

Within this general framework, certain limiting assumptions can be made to set up an admittedly oversimplified Gestalt formulation.

Our perceptual field contains objects and relationships between objects which are found in the geographical environment. A psychophysical brain field is postulated in which these perceived objects and relationships are isomorphically represented—the characteristics and relationships between the brain processes being functionally similar to those of the perceived objects to which they correspond. Thus, the isomorphic representation of objects, like their perceptual counterparts, have figural character: they are conceived as differentiated, bounded regions separated by an amorphous, labile, and extended ground.

Among the objects of the geographical environment usually represented in the perceptual field is the organism itself. Many of its characteristics, such as its spatial location, internal condition, and so forth, are likewise conceived to be represented in the brain field by a bounded, differentiated region. This subsystem within the perceptual field is in a sense the "cognized Ego": it defines the spatial framework and the position of the organism relative to the rest of the field.[4] It is, however, only cognized, not cognizing; while being an extremely important subunit of the field, it is nevertheless dependent upon the total field organization, and must come to equilibrium within it as any other subsystem does.

The perceptual field, as here described, is a dynamic system of stresses always tending to achieve equilibrium between its internal forces. When unbalanced tensions arise between various regions of the field, changes in form and organization occur which tend to restore the balance. If the percepts involved are geometrical figures of no particular thing-like character, the equilibration may be rather striking, as in the geometrical illusions and completion effects.

However, limitations upon such equilibratory processes are found even here. While completion phenomena are most pronounced in the rather unstructured situations of low illumination, projection onto the blindspot, and so forth, they are hardly noticeable under optimal conditions for analytic perception. Factors corresponding to what Gestalt theory calls "restraining forces" oppose the unbridled sway of the equilibratory tensions.[5]

Strong as these restraining factors may be in cases of plane geometrical figures, they assume even greater magnitude when objects or things are considered. For in dealing with objects in a three-dimensional articulated spatial environment, the overwhelming degree of "thing-constancy" bears witness to the powerful restraints exercised by the spatial framework upon the action of the "internal" equilibratory forces of the subsystems of the perceptual field. The equilibratory forces of such subunits must therefore involve the participation of the motoric (the activity of which is postulated to be at the service of perceptual equilibration), which alone can change the rigid restraint imposed by the spatial framework by an actual change of the geographical environment.

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[4] No better description of this role of the Ego could be presented than one by Köhler: "'In front,' 'to the left and right,' 'behind,' and 'above and below' are characteristics of an object which serves as the origin of the system of spatial coordinates. . . . This object (the Ego) . . . determines fundamental space aspects" (**3**, p. 322).

[5] Naturally, these restraining forces have their origin in the over-all tension conditions of the total field; lumping them together under the heading of "counter forces" is merely the result of abstracting a particular subsystem of the perceptual field and considering the rest of the field primarily in terms of its effect upon this subsystem.

Tension found within the perceptual field may involve stresses between the isomorphic representations of various objects or stimulus configurations of the environment; tensions can also arise between the isomorphic representation of the "ego" and that of another object or objects. "Motivational" situations are thought to involve such Ego-object tensions. In cases of positive motivational situations, the representation of the object and that of the Ego are considered to be subject to conditions making for unit-formation; that is, there is a force between the two corresponding bounded regions tending to bring them together in the brain field for maximum equilibration.

This equilibration will be manifested as locomotion with reference to the goal-object if the spatial constancies are high, which is usually the case. However, if the constancies are low, as a result of reduced stimulus conditions or any other factors that would decrease general field articulation, mere perceptual equilibration will become increasingly noticeable. Phenomena of this kind may correspond to the so-called "effects of motivation upon perception."

Equilibration may therefore be manifested as a purely perceptual coming together, even in the absence of actual locomotion. In other words the subject will tend to underestimate the distance to a "desired" object in proportion to the strength of his "desire"—the strength of the Ego-object tension—and in inverse proportion to the strength of the perceptual constancies.

The postulated Ego-object tension is viewed as arising from the field conditions themselves, as suggested above. It is therefore difficult to view it as a unidimensional line of force between the Ego and the goal-object; considering it as the resultant of a spatially extended total vector field seems a more reasonable procedure. Accordingly, the field would be affected even in regions adjacent to the central axis connecting Ego and goal-object. It is therefore predicted that test objects placed in these regions will be spatially distorted with reference to the medial line as well as to its terminals. At the moment no prediction can be made about the direction of this distortion. However, it seems plausible to suggest once more that the magnitude of the distortion will be a function of the constancy of that object, its distance from the medial line and its terminals, as well as of the strength of the Ego-object tension.

A series of experimental studies are now under way to determine the perceived distance of "desired" objects as compared with that of "neutral" ones, and to determine the spatial distribution of these effects upon both the goal-object and neutral test objects within the field. It is predicted that the results of these investigations will demonstrate the existence of a vectorial field in the whole region between Ego and goal-object and not confined merely to the medial line connecting the two. It is therefore expected that the perceived location of standard test pegs placed in a field containing a goal-object, will be distorted from its actual position in accordance with their position within this field. It is hoped that the quantitative relationships obtained will provide sufficient information to assign specific parameters and parametric relationships to take the place of the more general framework here presented.

The foregoing suggestions are clearly neither anything like a complete theoretical structure, nor the only alternative to a Separatist theory of motivation. They are, however, an indication of the possibility of a theoretical system, which treats both "cognitive" and "conative" processes as manifestations of the same underlying forces, and which subsumes motivational phenomena under the general organizational principles of Gestalt theory. In this system at least, there is no need for needs.

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TASK COMPLETION AS A FUNCTION OF ORGANIZATIONAL FACTORS*

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In this paper an attempt is made to study "motivated" behavior, not from the point of view of specific and relatively separate "drives" or "needs," but from the side of the structure of the subject's own psychological field. We should, for instance, be able to vary "motivated" behavior by varying the structure of the stimulus, since the stimulus itself is one of the important factors in determining the structure of the life space.

Specifically, I propose to investigate the operation in behavior of some of the Gestalt principles hitherto applied in the interpretation of perception and learning. If it is found that a "good" Gestalt in the perceptual field has a different effect upon "motivated" behavior from that of an indifferent Gestalt, then we can conclude that degree of "good" structuring of the field is an important variable in such behavior.

The logic of the present experiment can be stated very simply. The subject is presented with the problem of doing several simple tasks. Some of these tasks have a relatively definite internal structure, a sort of symmetry, and a clearly defined ending which is more or less dictated by the earlier steps in the task. Another set of these tasks is similar in the kind of materials used, but involves relatively discrete series of small parts whose last steps are not very different from the first ones. (See description below of the different tasks.) The procedure for both kinds of tasks is identical. By asking the subject to perform the tasks and then stopping him just before he completes each one, a tension is presumably introduced into the individual cognitive structures of these tasks. The hypothesis is that the tension introduced by interruption just before completion will be greater in the case of the "good" Gestalten, or well-formed tasks, than in the relatively formless ones.

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* I should like to express my thanks to Professor David Krech for his helpful encouragement throughout the experiment and for his aid in preparing the manuscript.

This is an attempt to set the phenomena which we ordinarily class as motivated behavior beside those which we ordinarily class as perceptual behavior in such a way that the underlying causes can be interpreted according to the same set of principles. The separation of the two types of concepts, "needs" and "perceptual Gestalten," is, I believe, a regrettable barrier to a really systematic psychology. As long as the different types of phenomena in psychology are kept in concept-tight categories, each using its own terminology, its own methods, and its own intervening variables, there can never be a complete psychology.

If we can meaningfully translate motivation into the type of concepts used in perception, then the variables which are important in perception must also be important in motivation. This has already been convincingly demonstrated in at least two widely separated fields of study. Mary Henle (2) reports experiments on the dynamic *and structural* determinants of substitution, which show that the factors of perceptual grouping, such as proximity and similarity, must be considered among the conditions under which substitution behavior will occur. David Katz (4) reports experiments of Bayer in which eating behavior of hens was shown to be greatly modified by differing perceptual conditions such as the amount of food available, the size of the grains, the number of other hens eating, and so forth. This behavior which is ordinarily thought to be dependent almost entirely on visceral conditions, can be by no means adequately accounted for on the basis of this one type of determining condition. The approach of the present experiment is still different, though the underlying aim is similar. Its intention is to demonstrate that if "gestaltedness" in a figure is a significant variable in how readily it is perceived as a unified figure, then "gestaltedness" in a cognitive structure should be significant in the production of tension.

This hypothesis has its basis in a deduction from a perceptual situation. Stimuli tend to organize themselves in the simplest, most regular pattern possible, and thus the gap in a not-quite-complete pattern will tend to be ignored, i.e., filled in in a manner structurally "demanded" by a "good continuation" of the *rest* of the pattern (6, p. 449). It should follow that if the stimulus has no pattern, or very little pattern, if the total effect is much the same whether or not certain parts are left out, then there should be no special tendency for parts which are left out to be filled in. If a task, including all the kinesthetic, visual, and tactual perceptions that go with performing it, can be thought of as analogous to a visual pattern, then a well-patterned task, one which has a definite ending with steps leading logically to it, should have a tendency to complete itself. If the activity of completing it is interrupted, the structure should "cry out" for completion somehow and the objective lack of closure should be a source of tension whose release would consist in the completion of the task. If, however, a task which had no distinct pattern as a whole and hence no special called-for ending were similarly interrupted, there should result relatively much less of this tension-for-completion.

This hypothesis finds some support in the results of several experiments with interrupted tasks. Zeigarnik (**11**) compared memory for interrupted tasks with memory for completed ones and found that under certain specific conditions the interrupted were remembered nearly twice as well. She concluded that the fact of having almost completed a task would, under these conditions, result in some kind of tension toward completion. This tension kept the task more readily available to recall. The theoretical structure I am proposing is similar to Zeigarnik's. She attributed better memory for certain tasks to this tension which existed because of the barrier to completion of these tasks. However, she wrote in terms of quasi needs, which made her discussion commensurate with motivational concepts of need. *I* would think of this tension as something which existed in a specific cognitive structure corresponding to the task and which differed from anything we would call a "need" just *because* of its reference to very specific cognitive content. And instead of saying that tensions are a kind of "need," I propose to say that "needs" generally could be interpreted as just this kind of tension, and to try to translate need theory into terms commensurate with concepts relating to the structure of cognitive content.

Two other of Zeigarnik's findings also contribute to my hypothesis. Using memory as a criterion of tension, she discovered that tension occurred more readily when the interrupted task was one in which each part built upon the previous part toward a definite goal than when the interrupted task was a monotonous series of additive subtasks (like making crosses on a paper) with no definite ending dictated by the inner structure of the task. If we can interpret her integrated tasks or *"Endhandlungen"* as "better" Gestalten than the additive tasks, then we can regard this as confirmation of our hypothesis.

Zeigarnik also found that tension was greater for tasks interrupted near their completion than for those interrupted earlier in the process. Presumably any task which is near completion is a better Gestalt than one which has only been begun. Interpreted in our terms, this result, too, brings us to the conclusion that the "better" the Gestalt of the perceived task, the greater will be the tension introduced in its cognitive structure when its completion in action is blocked just before it is achieved.

Ovsiankina (9) experimented also with "quasi-needs" (or tensions) which were presumably induced by interrupting a task before completion. Her criterion of tension, however, was not memory but resumption of the task. She reasoned that if a tension to complete the task was indeed set up, then the subject, if left in a free situation where it was possible to complete the task, should do so. She found this was true in varying degree depending on the circumstances. One of the circumstances that was relevant was the nature of the task and the moment of interruption in relation to it. She found that in the tasks which consisted of several well-defined subtasks, the tension was greater if the interruption came exactly at the end of this subwhole. In fact, she found it difficult to stop the subject before the end of the subwhole. He usually tried to finish just that part of the task. This relation of structural parts of the task to the effect of interruption is a further confirmation of my hypothesis that behavior can be shown to be influenced by what have been regarded as "mere" perceptual factors.

In the present experiment we are exploring these findings, adding two controls which were not before included. First, pairs of tasks were designed which were as nearly alike as possible except for the crucial differences in structure. The same materials and the same kind of manual operations were used for each member of a pair of tasks. Furthermore, any ego-involvement which might have been due to factors other than the structure of the task was controlled by interrupting both types of crucial tasks in the same way at a comparable point in their progress. The assumption is that the subject's cognitive structures of the task differ *only* in the formal qualities dependent upon the physical stimulus.

PROCEDURE

Subjects

The subjects were 33 college sophomores at the University of California. They were volunteers from an introductory psychology class.

Tasks

For each type of material two tasks were designed. There were three such crucial pairs of tasks. One pair, for example, consisted of stringing blue and yellow beads. The directions for the "good" task read: "String the blue and yellow beads as quickly as possible in the pattern illustrated below." For the formless task they read: "String the beads as quickly as possible." An illustration is provided with each, the one for the "good" task being colored and that for the formless task uncolored. Another pair of tasks consisted of putting snap fasteners together through holes in a card. One card had just enough holes for the number of fasteners provided, all arranged in a regular circle. The other, the formless task, had several holes more than there were fasteners and the holes were scattered randomly over the card. The third pair of tasks consisted of cutting a paper with scissors. One was a special design which opened into a large ring when completed. The other, the formless task, was just a series of separate and identical strips.

Since in this kind of experiment it has been demonstrated (**10**) that one of the conditions necessary for the Zeigarnik effect to appear is that not all the tasks be interrupted in any one series, three dummy tasks were added which subjects were allowed to finish. These were scattered through the series according to the choice of the subject, who determined the order. He did not know, of course, that some were to be interrupted. The dummy tasks consisted of filling in a little figure with crosses, making a chain of strips of paper and glue, and arranging numbered cards in order. For part of the experiment a fourth dummy task was included, which consisted of copying a child's drawing, but this seemed to bring in a distinct ego-orientation in almost all subjects, so it was dropped.[1]

Experimental Procedure

Subjects were brought into a small room with a table, several chairs, and a one-way screen. The directions for the nine or ten tasks, each typewritten on a separate sheet, were spread out on some chairs in the corner. The materials were on the work table. Subjects were told this was the "practice part of the experiment." They were told they would have a certain amount of time and were to spend it doing whichever they wished of the tasks in any order they chose. They were asked to read all the directions for all the tasks before starting on any one task. The experimenter sat across the table and kept up a conversation throughout, saying, "Don't bother to finish that one," or "That's enough, you don't have to do the rest of that," at specified points during the crucial tasks. Most subjects stopped either the first or second time they were asked. If they insisted upon finishing the task, it was simply counted as a completed task and not included in the results.

Most of the subjects regarded it as some sort of test of their manuel dexterity or an intelligence test for children, though in each case they were informed ahead of time that "This is not a test of any ability."

Just as the last task was completed, the experimenter said the time was up and explained that she was going into the next room to "set up" the rest of the experiment. She said this would take about ten minutes, adding that the subject might do "whatever he liked," that it didn't matter if he did anything with the materials on the table. The experimenter then entered the observation room next door and watched for exactly fifteen minutes.

At the end of this time the subjects were asked to come into another room and were handed a paper and pencil. They were asked to list whatever of the tasks they remembered.

The whole procedure took between 45 and 60 minutes.

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[1] Typical remarks made by subjects were: "Oh, I'm no good at drawing." "I'm not much of an artist." In general, subjects seemed to feel their ability was challenged much more than on the relatively mechanical tasks.

RESULTS

Resumption

It seemed possible that the order of resumption might be significant, as opposed to mere number of each kind of task resumed. In the first place, the more tense structures should tend to be resumed first. Secondly, since the tasks were paired, it seemed possible that doing one member of the pair might bring the other member more vividly to the attention of the subject and thus dispose him to resume that one, too. Either way, the first member of a pair (both of which were resumed) is probably more significant than the second. Likewise, the first task of all to be resumed is probably most significant.

If we consider only the first task resumed by each individual (Table I), we find a difference between the number of "good" tasks resumed first and the number of formless tasks resumed first which would not occur as often as once in a hundred times if the true probability for each being performed first was .5, that is, if there were no difference in tendencies of the two types of tasks to induce tension. It is plausible, then, to believe that a systematic factor favored the prior resumption of "good" tasks. Taking prior resumption as an indication of tension, we can conclude that more tension exists in an incomplete "good" Gestalt than in an incomplete structure which is relatively formless.

TABLE I

TYPES OF TASKS RESUMED FIRST BY EACH SUBJECT

Resumption Order	*Number of Subjects*	*Proportion*
Resumed a "good" task first	16	.89
Resumed a formless task first	2	.11
Total	18	1.00

More complete data on differential resumption of "good" and formless tasks are given in Tables II and III. However, it would be very difficult to subject these to statistical tests of significance.[2]

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[2] The number of tasks resumed by each subject can and does vary from zero to six. The separate tasks as tabulated, therefore, are not independent events (Table III). That is, three tasks resumed by one subject do not mean the same as one task resumed by each of three subjects, though in the tabulation of numbers of tasks resumed these two situations appear alike. Trying to test the significance of resumption differences on the basis of the number of subjects performing a larger number of each kind of task, we run into difficulty in deciding

We therefore present these raw data for inspection and rely on the tabulation of only the first task resumed by each subject to indicate statistical significance of these results. The fact that the largest number of subjects who resumed tasks showed a preference for resuming "good" tasks (Table II) indicates that tension was higher for these tasks. This hypothesis also appears to be supported by the data in Table III, which show that more than twice as many "good" tasks as formless ones were resumed by all subjects together.

TABLE II

RESUMPTION PREFERENCES OF INDIVIDUAL SUBJECTS

	Number of Subjects
Subjects who resumed one or more tasks	18
Resumed more "good" than formless	12
Resumed as many formless as "good"	5
Resumed more formless than "good"	1
Subjects who did not resume any tasks	15
Total	33

TABLE III

KINDS OF TASKS RESUMED BY ALL SUBJECTS

Type of Task Resumed	*Number of Tasks*
"Good"	27
Formless	13
Total	40

Tabulating further on order of resumption, it was found that in only one case was the formless member of a pair of tasks resumed without the "good" member also being resumed, whereas in 15 cases the "good" member was resumed but not the formless one. That is, for example, if the bead task without order was resumed, the ordered bead task was also resumed, but the ordered task might be resumed without the unordered one. Table IV shows the order of resumption within pairs where both "good" and formless tasks were resumed.

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what the expected probability is for number of subjects performing an equal number of each kind of task, since the total number of tasks varies so much from subject to subject.

TABLE IV

ORDER OF RESUMPTION WITHIN PAIRS OF TASKS

Order	*Number of Pairs*
"Good" task resumed first	11
Formless task resumed first	1
Total	12

TABLE V

TIME RELATION IN RESUMPTION OF PAIRS

Time Relation	*Number of Pairs*
"Good" resumed immediately after formless	1
Formless resumed immediately after "good"	8
Formless resumed after a delay	3
Total	12

Small numbers and lack of independence of the items would prevent our reliance on any statistical tests in this case, but it remains in this situation that any differential influence which order of resumption might have had on the two kinds of tasks would be such as to increase rather than decrease our confidence that "good" tasks have more tension. If resumption of one of a pair of similar tasks makes resumption of the other more likely, this factor will have operated more in favor of formless tasks than "good" ones.

Order of Performance

It might be suggested that the reason the subjects resumed the well-formed tasks was that these were more "interesting" to them. Without going into the problem of what is meant by "interesting" or whether in this case it could mean anything else than the relative "goodness" of the Gestalt, we can look at one finding of this investigation which indicates that more than some kind of vague "interest" is involved in the choice for resumption.

If the "good" tasks were somehow more attractive or interesting to the subjects than the formless ones, they should be expected to select the "good" tasks first in the original performance. They were given a free choice as to the order in which they were to do the tasks, and it was indicated that they might not have to do all the tasks. Taking pairs of "good" and formless tasks in order of performance, we find a difference too small to be significant. Apparently any preference for well-patterned tasks develops during the performance and is not something which becomes apparent in merely inspecting the directions.

Recall

We are not able to test the hypothesis that "good" tasks were more often recalled than formless ones since the small number of tasks made memory for all unfinished tasks almost perfect. Further, the fact that they were paired made it easier, probably, to recall the other member of the pair once the first one was recalled. We can, however, tabulate *order of recall* for the same two reasons (see page 198) which made us consider order of resumption significant.

TABLE VI

ORDER OF ORIGINAL PERFORMANCE AND OF RECALL WITHIN PAIRS OF TASKS

Member of Pair Performed First		*Member of Pair Listed First*	
"Good"	Formless	"Good"	Formless
32	31	42	21

Table VI shows that the "good" tasks were recalled more readily than the formless ones. Their earlier recall, according to Zeigarnik's interpretation, indicates that the tension to completion was stronger. Furthermore, if we can assume that the similarity of pairs of tasks did actually enter into recall, then we can believe that the memory for formless tasks has been more exaggerated by this factor than memory for "good" tasks.

In order to test the significance of the difference in order of recall the subjects can be divided into those who listed more "good" tasks of each pair first and those who listed more formless tasks of each pair first. The results are shown in Table VII. If the "good" and formless tasks were equally likely to be listed first, we could

TABLE VII

ORDER OF RECALL WITHIN PAIRS OF TASKS: BY SUBJECTS

	Number of Subjects	*Proportion*
Subjects listing more "good" tasks first	27	.82
Subjects listing more formless tasks first	6	.18
Total	33	1.00

expect this difference much less than once in a hundred times, the critical ratio being 4.73.[3]

SUMMARY AND DISCUSSION

It appears from all of the above data that behavior can be initiated, i.e., "motivated," by structural characteristics in the stimulus. With all nonstructural factors held constant by treating all crucial tasks alike, and with personality factors held constant by using the same person for each member of a pair of crucial tasks, a difference in the "goodness" of the Gestalt of a task has been shown to have a significant effect upon behavior toward that task.

There are at least two important questions which have not been so far mentioned. One is "what is the role of perception in differentiating our two types of tasks?" The other concerns the multitude of other factors beside the purely structural ones in the task itself which have to be considered in the type of situation we are dealing with.

If the difference between the crucial tasks is dependent upon the physical stimulus alone, we may ask whether the difference is a purely "perceptual" one. Is the tension for completion which we introduced by stopping the subject before he finished a function entirely of his perception of an unfinished task? Adler and Kounin (1) have attempted to answer this question. They presented children with two identical tasks, one half-finished before the experiment started, the other to be done entirely by the subject. The subjects were interrupted when they had done as much of their task as had been done on the other and were then left in a free situation where they could play with either. If the tension were entirely perceptual, there should be no difference between their performance toward the task which they had partially done themselves and the one which had been done before by someone else. Actually, nearly all the subjects finished the one they had been working on and almost none touched the other. Adler and Kounin interpreted this result as demonstrating that the concept of quasi need was a necessary one in the situation because something more than mere perception of an unfinished task was required to induce completion. I would accept the statement that resumption of interrupted tasks was a function of more than visual stimulation by an unfinished task, but I do not feel it necessary therefore to drag in the concept of need. The perception of a task which is merely seen partly done by another person and that of a task which the individual has himself worked on are themselves sufficiently different to warrant the prediction that they will influence behavior differently. It involves not only the visual perception and auditory perception (when the task is pointed to by the experimenter), but also all the multitude of perceptions and reorganizations that go with picking up the materials, deciding what to do first, planning the procedure and executing the task with one's own hands. Perhaps the well-formulated plan of how the task shall be done is the most important additional aspect of the task actually begun *by* the subject. The tension when this plan, which is the product of the subject's own imagination, is broken into would be much greater than what might be inspired by mere inspection of someone else's half-done work. Thus, it is true that the formation of such a tension depends upon more than functions which we would ordinarily lump under the heading "perception," for it goes into imagination, and action itself, but these further functions can be taken care of within the framework of another intervening variable, the "cognitive structure," and do not require us to borrow from "need" psychology. In short, our intention is not "to reduce everything to perception," but simply to point out that concepts which have been used primarily in the discussion of perception are applicable in situations not ordinarily thought of as "perceptual" and may serve as the common theoretical ground between phenomena that are traditionally separated into different fields.

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[3] In this situation we get out of the difficulty we had with testing the significance of the difference between subjects who resumed more "good" and formless tasks because there is no possibility of recalling an equal number of "good" and formless tasks first (there being only three pairs). Thus, with no "equal" category we can safely predict that half the cases will fall in each of the remaining categories provided there is in fact no difference in tension for the two types of tasks.

For the same reason it would not be necessary to maintain that the formal qualities in the stimulus of the task were the only or even the most important factors in determining memory or resumption of tasks. That not only the "perception" of tasks (in the limited sense), but the total context in which they appear enters into the subject's cognitive structure of the situation is vividly illustrated in an experiment of Marrow (7, 8) in which subjects were led by the directions to regard tasks which they had not completed as complete and vice versa. In this situation the "task" was to satisfy the experimenter, who stopped the work as soon as this criterion was reached. Thus, if he never stopped the subject, but allowed him to finish, then the "real" task, that of satisfying the experimenter, was not performed. In this situation the tasks which were never completed, i.e., those which satisfied the experimenter before completion, were less well remembered than those which were complete structurally but "dynamically" incomplete.

It has been suggested that mentioning these "other factors" beside the purely structural ones which influence behavior is a way of "letting needs in by the back door." The causal factors that are interpreted as need are, of course, being let in, though not by the back door. The condition of the viscera is undeniably a factor in behavior. What I am suggesting is that these conditions be reinterpreted, not as needs, but as changes in "cognitive structures" which are *also* conditioned by purely structural factors. The experiments on eating behavior of hens reported by Katz (4) which have been referred to earlier are a clear case of behaviorally identical effects of visceral and structural factors. Thus we would not have a separate *concept* of "need" which somehow "influenced" other essentially perceptual behavior units, but common intervening variables to mediate the effects of both visceral conditions and perceptual data.

Other factors are recognized as important in determining the total situation. However, the intention of the present experiment was to demonstrate that, where the necessary conditions for the Zeigarnik effect are fulfilled, the purely structural properties in the psychological field do play a role in behavior, the "goodness" of the Gestalt being a significant variable.

But from another point of view this experiment is more than that. It is an *illustration* of the application of concepts developed in the study of perception in its narrower sense to a broader behavioral situation. What I mean to say is that all the commonly separate areas of psychological theory can be united in a conceptual system whose intervening variables can be called "cognitive structures," or, if the word "cognitive" carries connotations which are not compatible with motivation problems, we can call the intervening variables "dynamic systems," as Krech (5) suggests. Either way, it is proposed that some common system can be established whereby the variables in motivation, learning, and perception are brought together theoretically. Psychology will never be *a* science until it abandons the "separatist approach," to use the words of Hochberg and Gleitman (3), and finds a way to reformulate principles from each of its "fields" so that the "total situation" can really be described as one dynamic whole which will suffice to explain the facts of motivation, perception, and learning.

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ON THE PERCEPTION OF INCONGRUITY: A PARADIGM

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Perceiving, for all its nicety of functioning in the dark room under strict instructions for accuracy, comprises a highly complex series of little understood psychological processes. For under all conditions, perceiving represents a resultant of two complex sets of specifications. One set describes the conditions of stimulation. This is done either in terms of physical measures such as wave length, or in terms of psychological norms such as in the description of a picture as that picture is seen by "normal" observers under optimal conditions and with a set for accuracy. This first set of specifications we are used to calling "stimulus" factors.

Stimuli, however, do not act upon an indifferent organism. There is never, in the old-fashioned language of G. F. Stout, anoetic sentience. The organism in perception is in one way or another in a state of expectancy about the environment. It is a truism worth repeating that the perceptual effect of a stimulus is necessarily dependent upon the set or expectancy of the organism. And so, in many situations the student of perception must also specify the expectancies of the organism when exposed to stimulation. If we sometimes, in simple sensory experiments, fail to do so, the reason is not that we do not care about the attitude of the organism but, rather, that we take it for granted that the observer is attentive to the task and that he is seeking to judge in terms of some required sensory dimension and not some other.

There have been very few systematic efforts to analyze the dimensions of set and to formulate laws regarding the effectiveness of set in perception such as those which describe stimulus-perception relationships. That students of nonsensory or "directive" factors in perception have thus far refrained from any large-scale statement of principles, while it is a mark of admirable modesty in the face of a very confusing array of experimental data, is highly regrettable.

For it has prevented the emergence of new hypotheses which, flowing even from premature principles, might serve to test the utility of theories of perception.

The present study, though empirical in nature, is essentially an essay in the theory of perception—or at least that part of the theory of perception which deals with directive factors in the perceiving process. Our basic axiom has already been stated—that perceiving is a process which results from the stimulation of a prepared or *eingestellt* organism. A second axiom concerns the operation of such directive factors: given a stimulus input of certain characteristics, directive processes in the organism operate to organize the perceptual field in such a way as to maximize percepts relevant to current needs and expectations and to minimize percepts inimical to such needs and expectations. This "minimax" axiom we have referred to elsewhere as the *construction-defense balance* in perceiving (1).

All of which is not to say that perception is always wishful or "autistic." Indeed, that is not the point. "Wishfulness" has to do with the nature of the expectations which are at work and is not a term relevant to the perceiving process as such. By "wishful" we mean an expectation with a low probability of being confirmed by events. The construction-defense process operates where expectations are "realistic" *or* where they are "wishful." In the former case, it is simply a matter of "constructing" a percept which is relevant, say, to the exigencies of locomotion, "defending" against percepts which, though potentially wish-fulfilling, are disruptive to the task of locomotion. While directive factors do operate in the interests of locomotion, the skier on the trail does not organize the distant steep hillside in a manner which would make his own path seem less steep and hazardous. Rather, in his perception, the dangers ahead may be sharpened and emphasized. Similarly, although it is the other side of the coin of "realism," one smitten by love does rather poorly in perceiving the linear characteristics of his beloved. His perceptual expectancies, so to speak, are elsewhere. The construction-defense balance operates along other lines. One sees softness of the skin, loftiness of brow, lightness of step—but perception of certain physical flaws does not materialize.

Perceptual expectancies, whether realistic or wishful, continue to operate so long as they are reinforced by the outcome of events. In short, expectancies continue to mold perceptual organization in a self-sustaining fashion so long as they are confirmed. It is when well-established expectancies fail of confirmation that the organism may face a task of perceptual reorganization.

The principal concern of this paper is with the perceptual events which occur when perceptual expectancies fail of confirmation—the problem of incongruity. Incongruity represents a crucial problem for a theory of perception because, by its very nature, its perception represents a violation of expectation. An unexpected concatenation of events, a conspicuous mismatching, an unlikely pairing of cause and effect—all of these have in common a violation of normal expectancy. Yet incongruities are perceived. Through a process of trial-and-check, to borrow a phrase from Professor Woodworth (8), the organism operates to discover whether any given expectancy will "pay off." It is either a very sick organism, an overly motivated one, or one deprived of the opportunity to "try-and-check" which will not give up an expectancy in the face of a contradicting environment.

It would be our contention, nonetheless, that for as long as possible and by whatever means available, the organism will ward off the perception of the unexpected, those things which do not fit his prevailing set. Our assumption, and it is hardly extravagant, is simply that most people come to depend upon a certain constancy in their environment and, save under special conditions, attempt to ward off variations from this state of affairs: "Thar ain't no such animal," the hayseed is reputed to have said on seeing his first giraffe.

Turning now to the specific study to be reported, our aim was to observe the behavior of intact, normal organisms faced with incongruous situations. How are such situations coped with perceptually? What is seen and under what conditions? We shall not be concerned with factors making for differences among individuals in their "tolerance for incongruity." Such studies are now being planned. It is essential first to examine *how* incongruity is dealt with, what repertory of responses is available to the organism in incongruous situations.

THE EXPERIMENT

Twenty-eight subjects, students at Harvard and Radcliffe, were shown successively by tachistoscopic exposure five different playing cards. From one to four of these cards were incongruous—color and suit were reversed. Order of presentation of normal and incongruous cards was randomized. The normal and "trick" cards used were the following.

Normal cards (printed in their proper color): five of hearts, ace of hearts, five of spades, seven of spades.

Trick cards (printed with color reversed): three of hearts (black), four of hearts (black), two of spades (red), six of spades (red), ace of diamonds (black), six of clubs (red).

Fourteen orders of presentation were worked out, and two subjects were presented the cards in each of these orders. There were three types of stimulus series: (1) a single trick card embedded in a series of four normal cards; (2) a single normal card embedded in a series of four trick cards; (3) mixed series in which trick and normal cards were in the ratio of 3:2 or 2:3. A summary of the orders of presentation appears in Table I. The reader will note that the average number value of the trick cards is slightly under 4 (3.94) and slightly over 4 for the normal cards (4.35)—a flaw

TABLE I

ORDERS OF PRESENTATION

Order	Card 1	Card 2	Card 3	Card 4	Card 5
Isolated "trick"					
1	*4H (B)*	5H	7S	AH	5S
2	7S	5H	*4H (B)*	AH	5S
3	5S	AH	7S	5H	*3H (B)*
Isolated "normal"					
4	5H	*2S (R)*	*AD (B)*	*3H (B)*	*6S (R)*
5	*6S (R)*	*3H (B)*	7S	*6C (R)*	*2S (R)*
6	*4H (B)*	*6C (R)*	*2S (R)*	*3H (B)*	5H
Mixed					
7	*2S (R)*	*4H (B)*	AH	7S	*6S (R)*
8	5S	*2S (R)*	*4H (B)*	*6S (R)*	AH
9	*4H (B)*	7S	AH	*6S (R)*	*3H (B)*
10	5H	*2S (R)*	5S	*6S (R)*	*4H (B)*
11	5S	AH	*6S (R)*	*4H (B)*	5H
12	*2S (R)*	5H	7S	AH	*4H (B)*
13	7S	*3H (B)*	*6S (R)*	AH	5S
14	*3H (B)*	5H	*6S (R)*	7S	AH

which operates slightly against the recognition of normal cards since lower value cards are probably more easily recognized.

Each card was presented successively until correct recognition occurred, three times each at 10 ms., 30 ms., 50 ms., 70 ms., 100 ms., 150 ms., 200 ms., 250 ms., 300 ms., 400 ms., 450 ms., 500 ms., and then in steps of 100 ms. to 1000 ms. If at 1000 ms. recognition did not occur, the next card was presented. In determining thresholds, correct recognition was defined as two successive correct responses. At each exposure, the subject was asked to report everything he saw or thought he saw.

The cards were mounted on medium gray cardboard and were shown in a Dodge-Gerbrands tachistoscope. The pre-exposure field was of the same gray color and consistency as the exposure field save that it contained no playing card. The light in the tachistoscope was provided by two G. E. daylight fluorescent tubes.

A word about the color of the incongruous cards is in order. Our efforts to have them printed by a playing card company were in vain. We therefore used poster paints to alter the colors of the cards. We had difficulty matching the red of a playing card, our best match being a slightly muddier and less yellow red than that of a regular card. Because of this, all red cards—trick and normal alike—were painted over in this color.

Results

Thresholds. Perhaps the most central finding is that the recognition threshold for the incongruous playing cards (those with suit and color reversed) is significantly higher than the threshold for normal cards. While normal cards on the average were recognized correctly—here defined as a correct response followed by a second correct response—at 28 milliseconds, the incongruous cards required 114 milliseconds. The difference, representing a fourfold increase in threshold, is highly significant statistically, t being 3.76 (confidence level $<.01$).

The threshold data, expressed as the cumulative percentage of stimuli correctly recognized as a function of increasing exposure time, are presented in Figure 1. The curves, generally, are parallel. The reader will note that even at the longest exposure used, 1000 ms., only 89.7 per cent of the incongruous cards had been correctly recognized, while 100 per cent of the normal cards had been recognized by 350 milliseconds.

Our design was such that we might test the hypothesis that the more experience a subject had had in the past with incongruity, the less difficulty would he have in recognizing incongruity of a related nature. Indeed, this is tantamount to saying that when one has experienced an incongruity often enough, it ceases to violate expectancy and hence ceases to be incongruous. Experience with an incongruity is effective in so far as it modifies the set of the subject to prepare him for incongruity. To take an example, the threshold recognition time for incongruous cards presented before the subject has had anything else in the tachistoscope—normal or incongruous—is 360 milliseconds. If he has had experience in the recognition of one or more *normal* cards before being presented an incongruous stimulus, the threshold rises slightly but insignificantly to 420 milliseconds. Prior experience with normal cards does not lead to better recognition performance with incongruous cards (Table II). If, however, an observer has had to recognize *one* incongruous card, the threshold for the next trick card he is presented drops to 230 milliseconds. And if, finally, the incongruous card comes after experience with two or three previously exposed trick cards, threshold

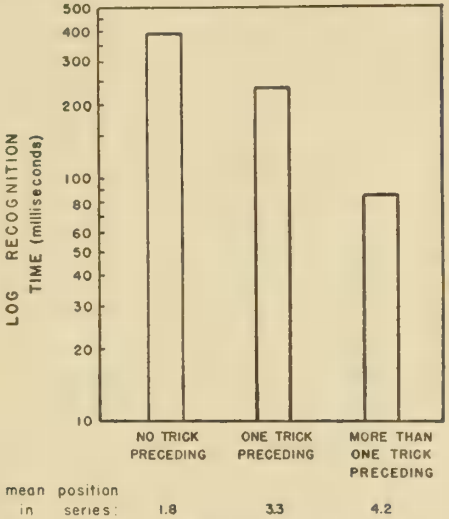

FIG. 1. Cumulative percentage of normal and incongruous playing cards correctly recognized as a function of increasing exposure time.

drops still further to 84 milliseconds. These figures, along with relevant tests of significance are summarized in Table III and plotted in Figure 2.

TABLE II

THE EFFECT OF PREVIOUS EXPERIENCE WITH NORMAL CARDS UPON THE FIRST RECOGNITION OF INCONGRUOUS CARDS

Previous Practice with Normal Cards	Mean Recognition Time	Number of Threshold Determinations	Mean Serial Position of First Incongruous Card
None	360 ms.	14	1
One or more normal cards	420 ms.	14	2.6

t = .38
P = .68

TABLE III

THE EFFECT OF PREVIOUS EXPERIENCE WITH INCONGRUOUS CARDS ON THE RECOGNITION OF SUBSEQUENT INCONGRUOUS CARDS

Nature of Previous Practice with Incongruous Cards	Mean Recognition Time	Threshold Number of Determinations	Mean Serial Position of Incongruous Cards
None	390 ms.	28	1.8
One card	230 ms.	22	3.3
Two or more cards	84 ms.	20	4.2

Tests of significance:

None vs. one	$t = 1.42$, $P = .16$
None vs. two or more	$t = 3.20$, $P = .001$
One vs. two or more	$t = 1.72$, $P = .08$

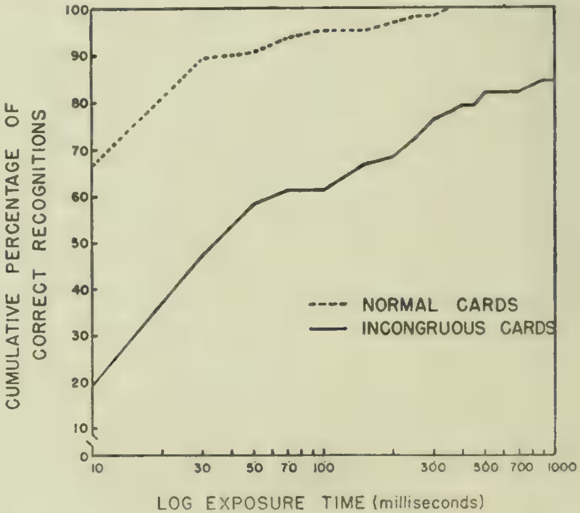

FIG. 2. Recognition thresholds for incongruous cards as a function of past experience with such cards.

The point immediately arises as to how much the decrease in threshold noted above is due to sheer number of trials in the tachistoscope (skill practice) and how much to the heightened expectancy of incongruity. Unfortunately our data are confounded in a complicated way here, and we shall have to depend upon rather broad inferences. We cannot partial out the differential effect of serial position of a card, whether first or third or fifth in the series independently of the kinds of experience the subject had before being presented any given card. Because of the nature of our design, such a procedure would leave us with groups differing in number and in the difficulty of cards presented. What evidence we have points, however, to the importance of previous experience with incongruity quite apart from skill practice. We have remarked already that previous tachistoscopic experience with normal cards serves to raise slightly the threshold for incongruous cards. Sheer skill practice cannot, then, be solely or even largely responsible for decreasing the threshold for incongruous cards.

Reactions to incongruity. We may ask properly at this point why the recognition threshold for incongruous stimuli was four times as high as the threshold for normal cards. The answer, it appears from an analysis of the data, lies in the manner in which subjects dealt with or coped with incongruity per se.

Generally speaking, there appear to be four kinds of reaction to rapidly presented incongruities. The first of these we have called the *dominance* reaction. It consists, essentially, of a "perceptual denial" of the incongruous elements in the stimulus pattern. Faced with a red six of spades, for example, a subject may report with considerable assurance, "the six of spades" or the "six of hearts," depending upon whether he is color or form bound (*vide infra*). In the one case the form dominates and the color is assimilated to it; in the other the stimulus color dominates and form is assimilated to it. In both instances the perceptual resultant conforms with past expectations about the "normal" nature of playing cards.

A second technique of dealing with incongruous stimuli we have called *compromise.* In the language of Egon Brunswik (2), it is the perception of a *Zwischengegenstand* or compromise object which composes the potential conflict between two or more perceptual intentions. Three examples of color compromise: (a) the red six of spades is reported as either the purple six of hearts or the purple six of spades; (b) the black four of hearts is reported as a "grayish" four of spades; (c) the red six of clubs is seen as "the six of clubs illuminated by red light."

A third reaction may be called *disruption.* A subject fails to achieve a perceptual organization at the level of coherence normally attained by him at a given exposure level. Disruption usually follows upon a period in which the subject has failed to resolve the stimulus in terms of his available perceptual expectations. He has failed to confirm any of his repertory of expectancies. Its expression tends to be somewhat bizarre: "I don't know what the hell it is now, not even for sure whether it's a playing card," said one frustrated subject after an exposure well above his normal threshold.

Finally, there is *recognition* of incongruity, the fourth, and viewed from the experimenter's chair, most successful reaction. It too is marked by some interesting psychological by-products, of which more in the proper place.

Consider now each of these four reactions, the forms they take, and the way in which they differentiate responses to normal and trick cards.

Dominance Reactions

A first datum is that 27 out of our 28 subjects showed dominance responses to the trick cards in their records, some considerably more than others. Strictly speaking, there is often no determinate way of discovering dominance reactions to a *normal* card. A correct response may be a dominance response—either dominance of color or dominance of form. A few instances of discernible dominance reactions to the normal cards were observed in the case of erroneous perceptions. There were, for example, instances in which a red normal card was seen as black and form was assimilated to match the black color. One subject saw the red five of hearts as the black five of spades for 8 out of the 25 exposures required for full recognition; another saw the red ace of hearts as the black ace of clubs for 3 out of 15 trials required for recognition. In sum, then, 7 per cent of the subjects showed dominance responses to the normal cards—attempting to rectify an incongruity imposed on themselves by seeing our imperfect red cards as black—while 96 per cent of the subjects showed dominance reactions to the incongruous cards.

The nature of the dominance reactions to the trick cards is easily described. First, such reactions occurred with equal frequency to trick black and trick red cards. Two options were available: either the subject could organize the field in terms of suit, e.g., hearts seen as red regardless of their stimulus color; or the field could be organized in terms of color, e.g., a red card seen as a heart or diamond regardless of its true suit.

It must be said, with whatever bearing it may have for Rorschach theory, that subjects showed a marked preference for one or another of the dominance options. Assuming that a fifty-fifty distribution of form- and color-dominant responses would be expected by chance, we tested for form and color types among our subjects. Of the 27 subjects showing dominance responses, 19 showed preferences for form or color in excess of chance at the .05 level or better; the remaining 8 being equally prone to both kinds of response.

To sum up, dominance reactions to trick cards were almost universal among our subjects. An incongruous stimulus was rendered congruent with expectancy by the operation of either form or color dominance. Only a small fraction of the responses to the normal cards was of this type; and where such responses occurred, they were always elicited by incongruities imposed by the subject through a failure to perceive correctly one of the attributes of the normal cards—e.g., perceiving a red card as black.

Compromise Reactions

As we have already noted, a compromise perception is one in which the resultant perception embodies elements of both the expected attribute and the attribute provided by stimulation. Compromise reactions are, of course, limited to certain types of stimulus situations where a "perceptual middle ground" exists between the expectancy and the stimulus contradicting the expectancy. Our situation was one such. The subjects often perceived color in such a way as to make it more in keeping with, or to bring it nearer to, normal expecation about what colors go with what suits. Perhaps the best way to illustrate the point is to list the different colors reported when subjects were presented with red spade and club cards.

Brown
Black and red mixed
Black with red edges
Black in red light
Purple
Black but redness somewhere
Rusty color
Rusty black
Lighter than black, blacker than red
Black on reddish card
Olive drab
Grayish red
Looks reddish, then blackens
Blackish brown
Blurred reddish
Near black but not quite
Black in yellow light

Several questions arise at once. To what extent can the color compromise reactions be attributed to the action time required for the development of red? To what extent is compromise color due to the fact that our cards were not precisely "playing card" red in color? Answers to these questions can be provided in two ways.

First, the *normal* red cards (hearts and diamonds) were colored in precisely the same manner as the incongruous red cards (trick spades and clubs). This being the case, we would expect (if action time or off-color alone accounted for compromise) that there would be no difference between normal and trick red cards. There is, however, a striking difference. In reacting to trick red cards, 50 per cent of the subjects showed compromise responses; only 15 per cent showed such responses to the normal red cards. It should be noted, moreover, that if action time were a major factor, the normal cards, recognized on the whole at briefer durations, should have been more susceptible of compromise.

We have also checked on the *frequency* of compromise responses per card for those cards which elicited any compromise responses at all. When compromise responses do occur, will there be a difference in their frequency for normal and incongruous cards? Using percentages of exposures on which compromises occurred, weighted by total number of trials to recognition, we find that the percentage of compromises elicited by incongruous cards averages 63 per cent of the exposures preceding correct recognition. On the other hand, those normal cards which produced compromise responses did so on the average in only 32 per cent of the prerecognition exposures.

In sum, then, it seems highly unlikely that either "off-coloredness" or action-time alone accounted for these interesting responses. The question remains, of course, as to whether these two considerations *facilitated* the appearance of compromise. Although we lack publishable evidence on the point at this time, it seems to us on the basis of our own direct observations of briefly presented colors that both are important. Color, briefly presented, is subjectively more labile; one is somewhat less subjectively certain about its hue, and saturation is less. Given the decreased stability of the color in our briefly presented cards, it is not surprising that the normal expectation should readily lead to compromise. And when, moreover, the card falls somewhat short of "playing card redness," the tendency toward partial assimilation may become even greater.

Compromise reactions to black cards were considerably rarer than such reactions to the red cards. When they did occur, they were always given to trick black cards, never to normal ones. Only 11 per cent of the subjects showed such responses. Where they did appear, they constituted a weighted proportion of only 12 per cent of the prerecognition trials. The quality of the compromise responses to black trick cards was not strikingly different from that already reported for red cards.

Grayish tinged with red
Black with reddish gray background
Dark red
First black, then red, then black

Why so few compromise responses to the black cards? Several highly conjectural answers are worth examining. The first, and perhaps the most likely, has to do with the greater phenomenal stability of black at rapid exposure. There is, to be sure, a certain graying effect if the black is figural on white, which probably has to do with the poorly defined boundaries of such a figure at brief exposure. But the gray does not have the same quality of lability or instability of the chromatic card when presented for a brief duration (e.g., 10-50 ms.).

What, precisely, is involved in perceptual compromise? The most parsimonious assumption, we think, is that we are dealing with a special case of color assimilation, viz., assimilation to expectancy. The effect has been noted before, although it may not have been called "compromise." For example, Duncker (3) reports that a green felt leaf in hidden red illumination maintains its greenness *better* than an identically colored felt donkey in the same illumination. It is apparent from his subjects' color matches that *neither* stimulus object maintained green *perfectly*, but that the leaf did so *more* than the donkey. The color wheel used as the variable stimulus for matching contained green, orange, and white sectors. The green sector for the leaf was 60 degrees, for the donkey, 39 degrees. Like us, Duncker was dealing in his experiment with the problem of "expectancy color"—i.e., that leaves are green—and as in our case his subjects were compromising between an expected color (leaf green) and a given color (the resultant of leaf green in hidden red illumination). Very probably other experimenters, to name Grace Heider (5) and Fuchs as but two examples, obtained color compromises at least in part dependent upon their subjects' expectations about the color of the objects being partially obscured by a differently colored episcotister.

DISRUPTION

Disruption is a gross failure of the subject to organize the perceptual field at a level of efficiency usually associated with a given viewing condition. Let us say that for *normal* cards the subject is able to perceive the color, suit, and number at from 20 to 50 milliseconds. The same subject, faced with an *incongruous* card at 50 milliseconds may just begin reporting with some degree of confidence and only partial accuracy on the number and suit and color of the card before him. This may go on for several exposures as duration is increased, let us say, to 100 milliseconds—well above his normal threshold range. At this point, the subject "loses confidence," becomes perceptually confused. Said our most extremely disrupted subject at 300 milliseconds in response to a red spade card:

> I can't make the suit out, whatever it is. It didn't even look like a card that time. I don't know what color it is now or whether it's a spade or heart. I'm not even sure now what a spade looks like! My God!

Disruption was not frequent in terms of the number of exposures on which it appeared. But it did occur in 16 of our 28 subjects in response to trick cards. Among these 16 subjects and for those cards on which disruption occurred at all, it occurred on the average in 4 per cent of their prerecognition responses (percentages weighted by number of trials preceding recognition). Disruption typically occurred after the subject had "tried out" his available hypotheses and failed to come to a satisfactory recognition. In Woodworth's terms (8), the trial-and-check procedure had failed to yield a stable percept.

The kinds of disruption varied from subject to subject and even from card to card. In analyzing disorganized reactions, one can find any one or any combination of five disruptive effects. One might well expect that disruption would be selective, affecting those attributes of the stimulus with which the subject was having maximum difficulty. Indeed, the figures above point to such selectivity—form and color being the primary loci of incongruity. Having exhausted his resources in trying unsuccessfully to perceive the incongruous pairing of form and color, the subject might indeed be expected to end in the plight characterized by the typical remark:

Color disruption	4 S's
Form disruption	10 S's
Form-color disruption	3 S's
Number* disruption	2 S's
"Corner"** disruption	3 S's

"I'll be damned if I know now whether it's red or what!"

But what of number disruption? How to account for it? Only two subjects, to be sure, seemed to show disruptive uncertainty about the number of pips present—in both cases after they had already perceived number correctly. In these instances one had the impression that the subjects had, so to speak, displaced their uncertainty upon an attribute of the stimulus which was not causing the real "perceptual trouble." Indeed, an additional exploratory experiment has indicated that in an extremely difficult incongruity situation such "displaced" disruptions are more common than in the present experiment. In that experiment a series of normal cards had interspersed at random among them two rather fantastic cards procured from a magic supply house: a card containing eleven diamonds but labeled in the corner with the notation for the fifteen of diamonds; the other was made up in the same way but was in the suit of spades and bore the notation of the fourteen of spades. In response to both cards subjects not infrequently ran the gamut of displacement: first being uncertain, properly, about the number, then suit, then color, then (though not frequently) about the size of the card.

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* Number disruption is a loss of perceptual certainty about the number of symbols on the card.

** Corner disruption refers to confusion about what is in the corner of the card, e.g., reports that the pip in the corner is "wrong" or displaced or that the number in the corner seems out of place.

Tempting though it might be to relate proneness to disruption to such concepts as Frenkel-Brunswik tolerance for ambiguity (4) or the concept of frustration tolerance (7), we do not at this time have any information which would warrant such an essay. One can, however, point to the phenomenon as a consequence of the "frustration" attendant upon failure to confirm a series of perceptual hypotheses. In this sense, the frustration is predominantly a perceptual matter (although some subjects grew irritated at their inability to "get" the stimulus).

A final word disposes of the frequency of disruption responses to the normal cards. There were none.

Recognition

In the perception of the incongruous stimuli, the recognition process is temporarily thwarted and exhibits characteristics which are generally not observable in the recognition of more conventional stimuli.

One specific way in which the recognition process is affected by the thwarting of well-established expectations is the emergence of a "sense of wrongness." The subject may either, even while "dominance" and "compromise" responses are continuing, suddenly or gradually begin to report that there is something wrong with the stimulus without being able to specify what it is that is wrong. It is not infrequent after such a report to witness the onset of perceptual disruption. But at the same time, such a "sense of wrongness" may also turn out to be a prelude to veridical recognition, for it often has the effect of making the subject change his hypotheses or give up his previous expectation about the nature of the stimulus.

Occasionally, as in 6 of our 28 subjects, the sense of wrongness may become focused upon a rather tangential, but, in point of fact, correct aspect of the incongruous stimuli and in so doing lead to a successful unmasking. These six subjects, prior to correct recognition, all reported that the position of the pips on the card was "wrong." All these responses were given either to spades printed in red or hearts printed in black at a time when the subject was calling the black hearts "spades" or the red spades "hearts."

What's the matter with the symbols now? They look reversed or something. (6SR)

The spades are turned the wrong way, I think. (4HB)

For those who do not have playing cards before them or who cannot remember the position of heart and spade pips, the former are printed with the point down in the top tier of pips and with the points up in the bottom, while spades are up-pointed in the top tier, down-pointed in the bottom.

Four of the six subjects who focused on this odd, and usually overlooked, positional arrangement of pips on a card finally achieved recognition. Two of the subjects failed to recognize their cards correctly in spite of unmasking this tell-tale feature.

Perhaps the greatest single barrier to the recognition of incongruous stimuli is the tendency for perceptual hypotheses to fixate after receiving a minimum of confirmation. As we have noted, some of our subjects persisted up to 1000 milliseconds in giving dominance responses to incongruous stimuli. Once there had occurred in these cases a partial confirmation of the hypothesis that the card in the tachistoscope was a black club or a black spade, it seemed that nothing could change the subject's report. One subject gave 24 successive black color-dominant responses to the black three of hearts, another 44 of them (both calling it the three of spades). Another persisted for 16 trials in calling it a red three of hearts. There were six instances in which subjects persisted in a color or form dominance response for over 50 exposures up to 1000 milliseconds, finally failing to recognize the card correctly.

Such fixation tendencies are, one might say, the chief block to perceptual learning. In another article on the effects of stress on perception (6), we pointed out that perceptual recklessness often resulted when a subject had to work under difficulties—the formation and fixation of "premature" and incorrect perceptual hypotheses. It would appear, indeed, that working in incongruous situations where partial confirmation of expectancy can occur (the form of a spade is not so different from that of a heart, even if the colors are) has the same effect of inducing premature fixation.

As for correct recognition of incongruity following an unsuccessful period of trial-and-check, we have irreverently come to call the response of some of our subjects the "My God!" reaction. For, to borrow a phrase from a distinguished literary critic, what occurs can well be characterized as "the shock of recognition."

One could, we suppose, liken the process of correct recognition to Köhler's description of insight. Indeed, it has some of the characteristics of sudden solution following unsuccessful attempts to master a situation. When a subject says: "Good Lord, what have I been saying? That's a *red* six of spades," there is no question about the sudden phenomenal emergence of the new perception.

Our reluctance in likening the phenomena to Köhler's description of insight is the suspicion that there is more to the matter than "sudden emergence." The uncertainty that sometimes comes before, the "sense of wrongness," the disruptions—all these point to the gradual weakening of previous hypotheses before "sudden reorganization" can occur. Indeed, to match cases of "sudden phenomenal emergence," one sometimes finds a very gradual and almost timed approach to the correct recognition of incongruous object color. A subject viewing a red spade may start by reporting a red tint which gradually becomes redder on succeeding trials until he finally asserts that the card is a red spade.

Unfortunately, we have no light to throw on this particular variant of the "continuity-noncontinuity" sequence in perceptual reorganization.

CONCLUSIONS

Our major conclusion is simply a reaffirmation of the general statement that perceptual organization is powerfully determined by expectations built upon past commerce with the environment. When such expectations are violated by the environment, the perceiver's behavior can be described as resistance to the recognition of the unexpected or incongruous. The resistance manifests itself in subtle and complex but nevertheless distinguishable perceptual responses. Among the perceptual processes which implement this resistance are (1) the dominance of one principle of organization which prevents the appearance of incongruity and (2) a form of "partial assimilation to expectancy" which we have called compromise. When these responses fail and when correct recognition does not occur, what results may best be described as perceptual disruption. Correct recognition itself results when inappropriate expectancies are discarded after failure of confirmation.

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PERSONAL VALUES AS DETERMINANTS OF PERCEPTUAL FIXATION

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The importance of perceptual, or cognitive, organization in learning has long been recognized by the Gestalt psychologists and has received special emphasis in the field of animal learning by Tolman (**20**). Whether one is inclined to accept substitution, effect, or expectancy as a basic condition of learning, the relevance of sensory organization in determining new patterns of reactions cannot be denied. We have only recently begun to investigate some of the culturally derived personality variables which condition the type of perceptual organization that the individual will achieve. One of these variables, personal value, has been shown to determine, at least partially, the selectivity of perception for value-words (**14**). Since individuals tend to perceive selectively in accordance with their basic values, or interests, it seems reasonable to suppose that they will also acquire new perceptual habits in a manner consistent with their particular value orientation. Demonstration of the role of personal values in perceptual fixation, or learning, is the purpose of the present study.

We have used the term *perceptual fixation* in the sense employed by Bruner and Goodman (**2**), who have suggested that perceptual behavior is characterized by three basic processes: *selection, accentuation*, and *fixation*. Selection refers to the lowering of thresholds for objects of distinct personal reference to the individual (**5, 11, 14**). Objects that are selected frequently become accentuated; that is, they appear brighter, larger, or more vivid (**2, 3**). Fixation denotes the persistence and preferential retention of certain selected percepts.

It seems proper to speak of perceptual fixation rather than perceptual learning, when we are considering sensory organizations, as a goal in itself rather than as a technique for acquiring new motor responses. Of the several studies dealing with the laws of learning in relation to perceptual organization, the principles generally stressed have been primary reward (15, 17) and frequency (7, 8, 10). At least three investigations, however, have considered the role of attitudes in the fixation of percepts. Seeleman (16) has shown that an unfavorable attitude toward the Negro tends to obliterate recognition of differences among Negro pictures, whereas a favorable attitude tends to heighten recognition of differences. Students holding stronger prejudices against the Jews were found by Allport and Kramer (1) to judge more faces as those of Jews and to be more frequently correct in their judgments than less-prejudiced students. Levine and Murphy (12) have demonstrated that anti-communist students learn and retain anti-Soviet material more readily than do pro-communist students.

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We have tried to devise a situation in which perceptual fixation may be demonstrated as a function of personal values without the specific mediation of primary re-enforcement or differential frequency of exposure to the perceived material.

Experimental Procedure

Neither selectivity nor fixation in perception can be measured directly; they can only be inferred from certain overt reactions taken as adequate criteria. We have considered strength of fixation to be systematically represented by rate of learning to associate a percept with its value denotation. For this purpose we first measured the value orientation of 24 undergraduates in psychology, 12 males and 12 females, by scores on the Allport-Vernon Study of Values. As symbols of the 6 values, 12 head-and-shoulder portraits of men were selected from *Time* and *Newsweek* magazines. None were distinguished by unusual facial characteristics or expressions, and none were sufficiently prominent as to be identifiable out of context by the authors or their associates.

Symbolic value was specified for each of the faces in terms of occupational titles presumed to represent fairly closely the six value areas as commonly conceived. The following pairs of values and occupational titles were used: *theoretical*—scientist, *economic*—businessman, *aesthetic*—artist, *social*—social worker, *political*—congressman, and *religious*—minister.[1]

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[1] While the term "clergyman" would have been more general, it was felt that "minister" would have more meaning in the specific cultural context. (As suggested by Mr. Bowles, "preacher" might have implied religious value even more effectively.)

The faces were shown to the observers in a Gerbrand's Mirror Tachistoscope at an exposure duration of .10 seconds, with an interval of approximately 8 seconds between successive exposures. During the initial series each exposure was prefaced by the statement from the experimenter: "This is a scientist (or artist, clergyman, etc.)." Since 12 faces were used to symbolize the 6 values, each value area was represented by two pictures. The entire series of pictures was presented in a predetermined random order, which varied from trial to trial so as to eliminate any primacy-finality effect that might otherwise have favored certain members of the series. As initial presentation of the 12 pictures was of special importance, each represented value appeared in each of the sequence positions four times. Following the initial run of 12 exposures, the observers were told that they would be shown the faces again and that they were to attempt correct identification of each face as it was presented. When a correct identification was made, the experimenter said "correct" and presented the next face. If the observer made an incorrect identification, the experimenter stated the correct one. The entire series of pictures was presented until the observers had correctly identified each face on three consecutive trials.

The pairings of faces with values was different for every subject. However, value designations were planned so that each face represented each of the six values for four subjects. Configurational aspects of the faces which might have facilitated their recognition were thus randomly distributed among the value areas as well as among the observers.

Results and Discussion

For each observer, we computed two statistics: (1) the rank-order correlation between values, ranked from one to six in order of the observer's preference, and the number of exposures necessary to establish correct recognition of the two faces representing each value; (2) the product-moment correlation between actual scores on the Allport-Vernon scale and the number of trials needed for fixation of the two faces representing each value.

Table I shows the computed *rho's* and *r's* between value and trials-to-fixation for each observer. Since *r* is not normally distributed and cannot, therefore, be subjected directly to analysis by the *t*-test, we converted all *r's* to *z*-scores following the method sug-

TABLE I

Correlations and Corresponding z-Scores between Value Scores and Mean Trials-to-Fixation for All Observers

Subject	*r*	*z*	*rho*	*z*
1	−.628	− .74	+.67	+.81
2	+.233	+ .23	−.31	−.32
3	−.630	− .74	+.69	+.85
4	−.103	− .10	.00	.00
5	−.159	−. 16	+.27	+.28
6	−.311	− .32	+.41	+.44
7	+.011	+ .01	−.10	−.10
8	+.529	+ .59	−.21	−.21
9	−.735	− .94	+.67	+.81
10	−.858	−1.28	+.71	+.89
11	−.434	− .46	+.59	+.68
12	+.214	+ .22	−.17	−.17
13	−.006	− .01	+.20	+.20
14	−.649	− .77	+.56	+.63
15	+.145	+ .15	−.38	−.40
16	−.711	− .89	+.61	+.71
17	+.664	+ .80	−.43	−.46
18	+.310	+ .32	−.13	−.13
19	−.441	− .47	+.71	+.89
20	−.501	− .55	+.60	+.69
21	−.352	− .37	+.43	+.46
22	−.451	− .48	+.29	+.30
23	+.657	+ .79	−.61	−.71
24	+.004	+ .00	−.07	−.07
		5.17		6.21
	$M_z = .215$		$M_z = .259$	
	$t = 1.92$		$t = 2.59$	
	$.05 < P < .10$		$.01 < P < .02$	

gested by Snedecor (18). Since the difference between *r* and *rho* is not great for small arrays, we took the liberty of also converting the *rho* values to *z*-scores as if they were *r's*. This deviation from orthodoxy is probably preferable to direct statistical analysis of the distribution of *rho's*, in view of the fact that they are not distributed normally. For each array of *z*-scores, thus computed, we tested by means of Fisher's *t*-technique the significance of the deviations of the mean correlations from zero. As may be noted in Table I, the mean *z*-score based on *rho* for all twenty-four observers violates the null hypothesis at the .02 confidence level. The mean *z*-score based on *r* barely misses significance at the .05 confidence level. However, viewing the statistical findings as a whole, we feel that a positive relationship between individual value preferences and rate of perceptual fixation for objects denoting those values is clearly indicated.

One further statistical operation was performed on the data. We first totaled the trials required for all observers to fixate the faces representing their highest ranking value, their second-ranking value, etc., down to the lowest ranking value of each (Table II). An analysis of variance was then undertaken to determine whether the means of the six value ranks differed significantly. An F value of 2.37 was obtained, indicating significance at the .05 confidence level.[2] Value rank apparently introduced a real difference into the mean number of trials required by the observers as a group to fixate pictures symbolizing different value areas (Table III).

TABLE II

AVERAGE NUMBER OF TRIALS REQUIRED FOR THE OBSERVERS TO LEARN FACES REPRESENTING DIFFERENT RANKING VALUES

Value rank	1	2	3	4	5	6
Average trials	6.4	8.2	8.7	9.4	7.7	8.9

TABLE III

ANALYSIS OF VARIANCE

Source	Degrees of Freedom	Sum of Squares	Mean Square
Total	143	3065.28	21.44
Ranks	5	131.72	26.34
Individuals	23	1653.56	71.89
Remainder	115	1280.00	11.13

F (individuals) $= 6.46$ $P < .01$
F (ranks) $= 2.37$ $P < .05$

Perceptual defense versus selective vigilance. In Figure 1 we have plotted the mean trials-to-fixation for the ranked values of the group, considering in each case the individual value preferences of the observers. As may be noted from the figure, the general trend is toward an increase in trials to fixate lower ranking value-percepts. Value-rank 5, however, presents a striking exception to this trend, since no more trials were needed to fixate faces representing this rank than were needed for rank 2. We believe that this irregularity in the general function is due to the lumping together of individuals who showed a negative correlation between trials and value rank with those who showed positive correlations. In at least two previous studies (**4**, **14**), it has been observed that while most individuals display greater sensitivity to *valued* stimuli, a few always show a reversal of this pattern, that is, a lowered threshold of receptivity to *inimical* stimuli.

[2] This F value, as suggested by Dr. Oliver Lacey, confirms the previous analysis but cannot stand by itself because of the inflated number of degrees of freedom introduced by repeated measurements on the same individuals.

While we are not prepared at present to defend the proposition that two basic and distant types of perceptual response—perceptual defense and selective vigilance—will appear consistently in the same observers, we do suggest that this possibility should not be overlooked in future research. To indicate the kind of difference that may be found, we have plotted separately in Figure 2 the learning functions for the fifteen observers showing negative *r's* and the nine observers showing positive *r's* between value scores and fixation trials.[3] It at once becomes apparent that the drop in the curve at value-rank 5 in Figure 1 is the result of the greater readiness with which nine observers fixated faces symbolizing their lowest values. This tendency has been noted previously by Postman, Bruner, and McGinnies (**14**) and has been referred to by Bruner and Postman (**3**) as "selective vigilance," a process of sensitization to poten-

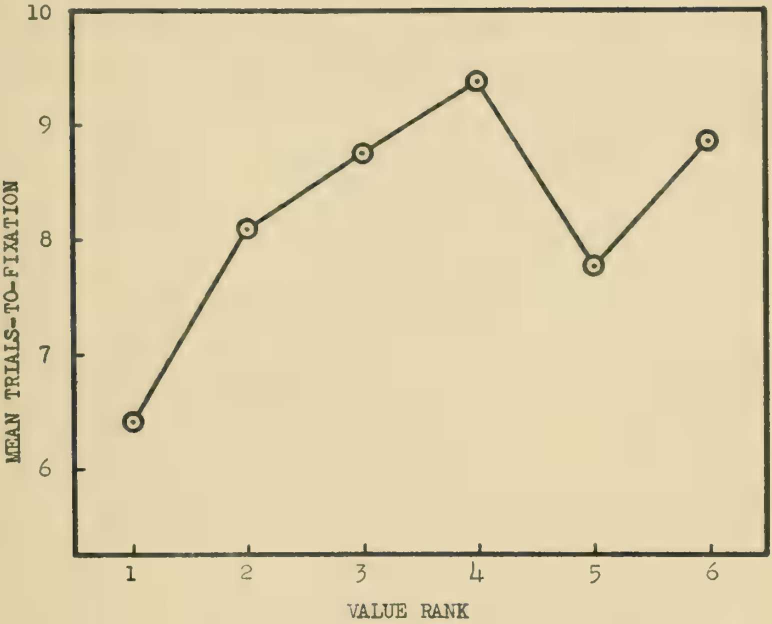

FIG. 1. Mean trials-to-fixation for the group of observers according to rank of the value-percepts. Rank 1 represents the highest values of the group, rank 6 the lowest.

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[3] Since fewer fixation-trials in general were required for faces associated with high values, this relationship is indicated by *negative r's* between fixation-trials and value scores. When the values are ranked from highest (rank 1) to lowest (rank 6), *positive rho's* indicate more rapid fixation of faces related to high ranking values.

tially dangerous stimuli which to some observers is a more adaptive type of response than the "perceptual defense" generally displayed. If it can be demonstrated that observers will adhere consistently to either defense or vigilance when faced with little-value or threatening stimuli, the implications for perceptual theory will be significant indeed.

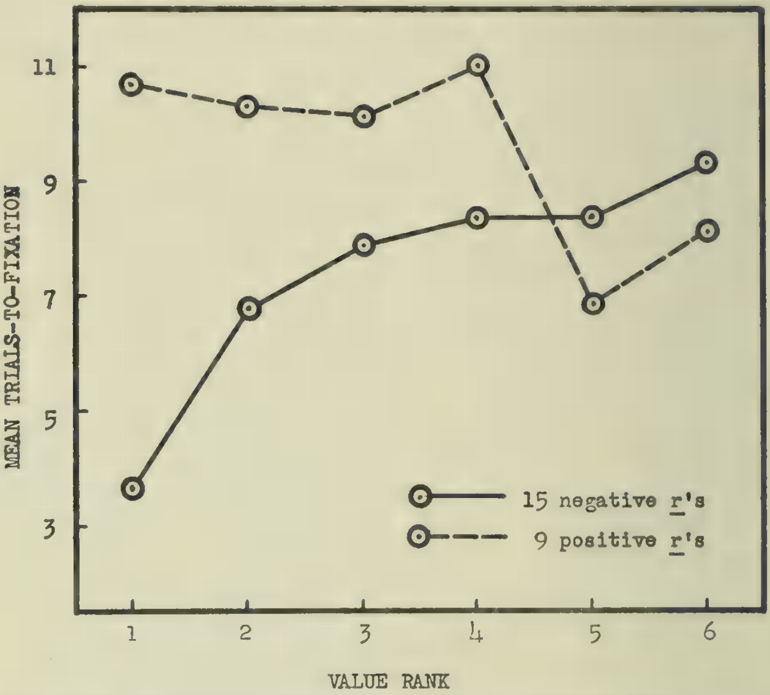

FIG. 2. Mean trials-to-fixation according to value rank for fifteen observers who showed more rapid fixation of high-ranking percepts and nine observers who fixated more readily low-ranking percepts.

Perceptual fixation and learning. To say that learning in humans is facilitated by interest or attitude is to leave unstated the mechanism by which such facilitation is accomplished. In the present study we have investigated the role of a specific group of attitudes—personal values—in perceptual fixation. The results pose a challenge to learning theorists.

We have already acknowledged Tolman's contribution to learning theory through his emphasis on perceptual organization as the process by which new cognitive patterns are acquired. Employing

Tolman's conceptual framework in reference to the present results, one might say that individuals have through experience acquired "expectancies" for value-related symbols. Such symbols have become "signs" leading to the "significates" or goals of value-related objects or activities. In order to explain the differential ease with which certain of these sign-significate relationships are established, however, recourse must be made to one of Tolman's "dynamical laws," namely, the law of motivation. Spence (19) has quoted Tolman on this point as follows: ". . . if some one of the resultant f's (significates) should be especially satisfying, the learning of the particular sign-gestalt, or even of the whole set of alternative sign-gestalts, would probably thereby be facilitated." We may assume then that when the religious observer perceives a "minister," that particular expectation is confirmed and is strengthened in proportion to the observer's motivation toward religious percepts. Similarly, each individual will tend to fixate different percepts with greater or less celerity according to the relevance of the percepts to his own particular value system. As Spence goes on to point out, however, "such a principle amounts . . . to about the same thing as the principle of re-enforcement, except that it refers to sign-gestalt-expectations rather than S—R connections."

Actually, by further reference to sign-significance terminology and the invoking of an additional principle of Tolman's, the law of emphasis, an explanation may be provided for one finding that might prove difficult of description in terms of re-enforcement; namely, the fact that some individuals fixated more rapidly the symbols of their lowest values. According to the principle of emphasis, certain emphatic or compelling stimuli may facilitate the acquiring of sign-gestalt expectations that are associated with them. If we assume that even unpleasant percepts are emphasized through their very "vividness" or "clarity," then the expectancies of certain observers for percepts low in their scales of values may be strengthened more than their expectancies for highly valued symbols which, in reality, may be less vivid or impressive to them.

While the foregoing discussion may not do complete justice to Tolman, the main lines of the argument appear representative of his position. The major weakness in this type of explanation lies in the necessity for invoking a need or motivational state to account for the differential ease with which certain of the faces or "signs" are fixated. If the preferential confirmations of certain expectancies, or value-oriented sets, are dependent upon motivational conditions, then the entire interpretation borders close upon re-enforcement theory. In fact, an appeal to reward learning theory, as represented by Hull's position, offers several positive advantages of description.

We may assume, first, that value preference, as measured by the Allport-Vernon scale, implies a disposition to react in a characteristic fashion to objects and symbols which relate clearly to value areas.[4] Such value preferences, or reactive predispositions, may be considered the end-products of reward learning. The individual has found certain value-related experiences to be rewarding; others have proved unrewarding or even punishing. Reacting to a value-symbol, of course, invariably follows perception of the symbol. Thus, through association with the consequences of value related activity, perceptions of value-symbols acquire secondary re-enforcing properties of either reward or punishment. Hull is explicit on this point:

> Careful observation and experiment reveal, particularly with the higher organisms, large numbers of situations in which learning occurs with no associated primary need reduction. When these cases are carefully studied it is found that the reenforcing agent is a situation or event involving a stimulus aggregate or compound which has been closely and consistently associated with the need reduction. Such a situation is called a secondary reenforcing agent. . . . (**9**, p. 387)

To the "theoretical" individual the perception of a "scientist" has acquired secondary reward value because of the association of the percept with rewarding "theoretical" activities. The perceptual response "scientist," therefore, is fixated more strongly because it has acquired the characteristics of a re-enforcing agent. We have tabulated the correct identifications made by the observers on the *first* trial following preliminary designation of the faces by the experimenter. The total number of correct identifications as a function of value rank yields a function strikingly similar, although inverse, to that presented in Figure 1. These data are summarized in Table IV, where the trend toward preferential fixation of high-ranking value symbols on the first recognition-trial is clearly evident. This may be considered as presumptive evidence for our contention that

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[4] For a discussion of value as a determinant of word association, for example, see McGinnies (**13**).

TABLE IV

Total Number of Correct Identifications According to Value Rank Made by the Observers on the First Recognition Trial

Value rank	1	2	3	4	5	6
Total number of correct identifications	26	22	16	15	21	16

the perceptual response serves as its own re-enforcement, and that fixation is not functionally dependent upon verbalization of the value symbol by the experimenter following presentation. Since verbal identification always *preceded* presentation of each face on the initial run, one might justifiably conclude that the face was actually perceived in accordance with the value designation. Otherwise, one could argue that any alien stimulus, whether it could actually be perceived as a "minister," or "scientist," etc., or not, might be re-enforced by the verbalization of a value designation immediately following exposure. Nonsense symbols, for example, might be differentially fixated according to value identifications given upon exposure of the syllables. In this case, it could be asserted that no unique perceptual experience has taken place, and that learning has proceeded in highly predictable fashion according to the postulates of re-enforcement theory.[5] The issue here, of course, is whether the percept or the identifying verbalization is the re-enforcing agent. It appears that only further research will clarify the issue.

Fixation as a perceptual mechanism. In a previous study dealing with the role of value in perception (**14**), it has been suggested that three mechanisms govern the perception of value-related symbols: *selective sensitization, perceptual defense,* and *value resonance.* The present results appear to be consistent with these postulates. Since the stimulus objects of the present experiment were presented to the observers at durations well above their limens of recognition, they could not achieve perceptual organizations consistent with their value orientation through depression or elevation of recognition thresholds. Selective sensitization then, when it cannot operate through a lowering of threshold, becomes apparent in the greater ease with which individuals fixate visual symbols of their preferred values. Perceptual defense, when it cannot be effective through a raising of thresholds, is manifested in greater difficulty of fixation. Value resonance, to be evident in the present study, would have required that the observers be able to make perceptual hypotheses. This they were unable to do since the identifications were determined arbitrarily by the experimenter. In addition, most of the subjects quickly ascertained that each occupational title was represented only twice in the series of faces and so did not guess more frequently along lines consistent with their value preferences.

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[5] Such an experiment is in progress under the direction of Dr. Paul S. Siegel, to whom the writers are grateful for several critical suggestions regarding interpretation of the present data.

SUMMARY

Twenty-four observers were required to identify correctly a series of 12 unfamiliar faces according to identifications given each face on its first exposure by the experimenter. The faces were presented serially in different random orders until the observer had correctly identified each one on three successive trials. Identifications were by occupational titles selected as grossly representative of the six Spranger values. Correlations were determined between the number of exposures required for each observer to fixate correctly the two faces representing each value and his scores for the corresponding values as measured by the Allport-Vernon scale. Individual rates of perceptual fixation, thus determined, were found to correlate significantly for the group with value scores. In general, the observers fixated more readily faces symbolizing their highest values than they did faces representing their lowest values. The results are interpreted as indicating that in the absence of primary reward or differential frequency of experience, fixation will occur more readily for percepts having secondary reward value acquired through association with value-related activity. In the presence of supraliminal stimuli, differential ease of perceptual fixation is an alternative method of achieving perceptual organizations that are acquired in an equivocal situation through selective sensitization or perceptual defense.

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THE EFFECT OF NEED FOR ACHIEVEMENT ON RECOGNITION OF NEED-RELATED WORDS[1]

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As part of a general research program on projective measures of motivation, McClelland, Clark, Roby, and Atkinson (4) have reported a study in which they derived what purported to be a measure of the need for achievement (n Achievement). In this study it was shown that experimentally induced failure under ego-involving conditions produces characteristic changes in brief, written Thematic Apperception Test (TAT) stories. Because of the nature of the experimental operations and because of parallel results with the hunger drive, it was argued that the changes in the stories reflected an over-all increase in n Achievement. Accordingly these changes have been used as the basis for a scoring system designed to measure the n Achievement of a subject who writes TAT stories under neutral conditions.

In an attempt to extend the meaning of this new measure of n Achievement, Clark and McClelland (2) undertook to find out whether it was related to performance, specifically in this case, to performance on an anagrams task. The anagrams task was patterned after one used by Thurstone (7), and allowed the subject 12 minutes to make as many words as possible out of the word *generation.* Clark and McClelland found that the TAT n Achievement scores *did* correlate significantly with the subject's output on the middle time section (fifth and sixth minutes) of the anagrams test. The TAT scores did *not* correlate with performance at the beginning or end of the test. It was thought interesting, in this connection, that although the output of the group as a whole decreased significantly in the middle time section of the test, the drop at this point was significantly less for those subjects who scored high on the TAT measure of n Achievement. It was as if the subjects who have high TAT n Achievement maintain their motivation and their level of performance even in the apparently most boring or least stimulating part of an assigned task.

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[1] This is No. 6 in a series of papers on the projective expression of needs made possible by a grant from the Office of Naval Research, for which the authors are very grateful. We are also indebted to John Perkins, David Angell, and Kenneth Wigren for assistance in the conduct of the experiment and preparation of the report.

In any event, we have now two correlated measures of n Achievement: one based on the TAT, the other based on performance in the fifth and sixth minutes of a 12-minute anagrams test. Furthermore, Clark and McClelland (2) report that these two measures largely define one factor in a factor analysis of achievement-related measures; that is, they are not only correlated with each other, but covary in their relation to other measures.

The purpose of the present experiment was simply to determine the relationship, if any, between these two measures of n Achievement, on the one hand, and the ease or readiness with which need-related words are recognized, on the other. That there may be such a relationship is suggested by the work of Postman, Bruner, and McGinnies (5), who have shown that the time required for recognition of tachistoscopically presented words is determined in part by the connotation of the words as they relate to the individual needs or values of the subject. By determining the relation, in this case, between n Achievement and recognition of need-related words, it may be possible to enlarge the meaning of the n Achievement measure, and also to throw light more generally on the way in which a need or motive influences readiness to recognize or perceive those parts of the environment which are relevant to the need in question.

PROCEDURE

The procedure involved, first, securing measures of n Achievement on the TAT and anagrams tests as described elsewhere (2, 4) for each of 36 Wesleyan undergraduates. About three months after these tests had been given, each of the same 36 subjects was put through a procedure designed primarily to determine the ease with which the subject would recognize achievement-related words. In general the procedure was similar to that used by Postman, Bruner, and McGinnies (5). Specifically, it was as follows:

Two judges selected a total of 30 words for use in this experiment. Ten of the words were intended to be neutral with regard to motivational or emotional connotation. Ten were related to achievement. And ten were related to security, this group of words having been included originally as an additional control. The achievement-related and security-related words were further broken down so that, in each case, three of the ten connoted deprivation, three instrumental striving, and four success or gratification. The actual words used are presented in Table I along with the five practice words and with numbers indicating the order of presentation. The groups of words are not all equated for familiarity in terms of the Thorndike Word Count (6), but subsequent study indicated that for the groups of words used in the final analysis, the average familiarity ranking was sufficiently close to make any interpretation of the results in terms of such differences highly unlikely.

TABLE I

WORDS USED TO MEASURE RECOGNITION TIME

PRACTICE	NEUTRAL	ACHIEVEMENT-RELATED	SECURITY-RELATED
costume	1. window	A. Deprivation words	
mention	4. believe		
flower	7. western	2. unable	3. lonely
animal	10. neutral	8. obstacle	27. anxiety
terrace	13. article	23. failure	29. threat
	16. horizon		
	19. actual	B. Instrumental words	
	22. position		
	25. confuse	6. compete	5. nurture
	28. reality	20. strive	15. belong
		30. achieve	18. loving
		C. Goal words	
		11. mastery	12. support
		14. perfect	9. friend
		17. success	21. comfort
		26. famous	24. security

NOTE: The practice words were presented first. The numbers indicate the order of presentation. There was a three-minute rest halfway through after *belong*.

Each of the words was typed in capitals on standard transparent slides for use in an ordinary slide projector, the projector being equipped with a shutter so that the words could be flashed on a screen for .01 sec. A fixation area was provided on the screen in the form of a rectangle, about 15 inches by 8 inches, and all images of the words appeared on the screen within this area. The subject sat normal to the screen and at a distance of about 15 feet from it. All subjects were run off individually. The instructions given were as follows:

This is a test of perceptual acuity, designed to determine how readily you can recognize words which are exposed for very brief periods of time. There will be a number of words to be identified. Each word will be presented over and over until you have correctly identified it, then we will go on to the next word.

Specifically, the procedure will be as follows: When I say "ready" you are to fixate this rectangle on the screen [point out]. About one second after I say "ready" a word will be exposed very briefly in that rectangular square. If you have no idea what the word is, say "No." If on the other hand you recognize or think you recognize the word, or any of its letters, tell me what they are. Please make your response quickly. In no case will you be allowed more than five seconds to make your response. One second before the next exposure I will say "ready," the word will be exposed, and you will again try to identify it. We will continue with this procedure until you have correctly identified the word. Then we will pass on to the next word.

Remember!

Be sure to fixate the rectangular area when I say "ready." If you don't, you won't see the word.

Don't hesitate to guess about the words or any of the letters if you think you know what they are.

Recognition thresholds were determined for each subject as follows: First, on the basis of five practice words, the experimenter determined the approximate value of lamp voltage at which the subject would recognize a neutral word when it was exposed for .01 sec. A value about 15 volts below this point was then selected as the setting at which to begin presenting each of the neutral and significant words. This value varied considerably from subject to subject. In fact, it was this variation which dictated the decision to try to begin with each subject at a specified distance below his threshold, as it was felt that extraneous factors such as fatigue and frustration might enter to complicate the results in the case of any subject who had to sit through many exposures before reaching a threshold. Beginning at the predetermined below-threshold setting, the experimenter would expose the first word and then increase the voltage by one volt and expose again, continuing thus to step up the voltage on the lamp and to expose the word until the subject had correctly identified it. In this way a measure was secured for each subject of the number of times the word had to be exposed, at progressively increasing brightnesses of the lamp, before the subject could identify it correctly.

In order to reduce inter-subject variability, all recognition time values for achievement-related and security-related words are expressed in relation to the speed with which the subject perceived the neutral words. Specifically, the number of exposures required for recognition of the need-related word or words is subtracted from the mean number of exposures required by that subject to recognize the neutral words, and the difference is divided by the S.D. of the number of exposures required for the neutral words. A plus score, then, means that the subject (or subjects) saw the word faster than the mean of the neutrals; a minus score means that he saw it more slowly than the mean of the neutrals.

Results

As a preliminary step the mean recognition times were calculated for the subjects scoring above and below the mean TAT n Achievement score and above and below the mean 5-6 minute anagram score. The results are summarized in Table II. Originally the breakdown was made separately for all three types of achievement-related words, i.e., deprivation, instrumental, and goal words. The instrumental words (*achieve, strive, compete*) behaved so much like the goal words that a two-way grouping into − words (deprivation words) and + words (instrumental and goal words) was

TABLE II

Mean Recognition Times in Standard Scores for Achievement-Related Words for Subjects Scoring Above and Below the Mean on TAT n Achievement and on 5-6 Minutes of the Anagrams Test

Subject Groups	TAT n Achievement Score				5-6 Min. Anagrams Score			
	N	All words	Three − words	Seven + words	N	All words	Three − words	Seven + words
Above mean	16	.00*	−.35*	+.19	17	−.01*	−.42*	+.20
σ_m	..	.10	.12	.11	..	.10	.12	.10
Below mean	20	−.18*	−.40*	−.09	19	−.16*	−.34*	−.08
σ_m	..	.08	.13	.07	..	.09	.13	.08
Difference	..	+.18	+.05	+.28	..	+.15	−.08	+.28
σ_{diff}	..	.13	.18	.13	..	.13	.18	.13
Critical ratio	..	1.38	.28	2.15	..	1.15	.45	2.15
P	..	....	....	<.05	..	....	....	<.05

*One subject omitted whose recognition time for one or more of the words exceeded six standard score units.

adopted.[2] As Table II shows, the subjects who were high and low on the two measures of achievement reacted quite differently to these two types of words. In general the high n Achievement group, by either measure, recognized the + words more quickly than the low n Achievement group, but the same relationship did not hold for the − words.

Attempts were made to refine the relationship between the two measures of n Achievement and recognition time by further breakdowns of the distribution into thirds and fourths, but the results, while suggestive, proved statistically unreliable in every case. Therefore on the grounds that the two measures show the same relationships in Table II and on the basis of previous work by Clark and McClelland (2), it was decided to combine the two measures into a single one which would represent a probably more reliable estimate of a given subject's level of n Achievement. Several different methods of combination were tried, all of which seemed to yield substantially the same results. The one finally adopted was simply to rank the scores on each measure of n Achievement, average the ranks for a given subject, and then rerank the combined ranks. The resulting combined rank distribution was then broken into thirds to plot more precisely the relationship between increasing n Achievement and recognition of + and − need-related words. The results are summarized in Table III and Figure 1.

Considering first the achievement + words, we note that there is no increase in sensitivity from the low to the middle n Achievement group, but that there is a large significant increase in sensitivity from the medium to the high group. In nonlinear relations of this sort, it has seemed most conservative to compute the t-values of the differences between the mean of each group and the other two means

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[2] A correlational analysis was attempted to discover whether the words which on a priori grounds were supposed to belong to a given group actually did covary empirically. It was found that the recognition times for the deprivation or — words (*unable, obstacle, failure*) had an average intercorrelation of +.46 as compared with one of +.33 with the other seven achievement-related words. The four goal words (*success, famous, perfect, mastery*) likewise had a higher correlation on the average with each other (+.50) than they did with the other six words (+.34), but the instrumental words apparently did not form a discrete group since they correlated the same on the average with each other as they did with other words in the list. Nevertheless, it was decided to throw them in with the goal words on two grounds: (1) they correlated slightly higher on the average with these words than they did with the deprivation words, and (2) they were all three recognized more quickly by the high TAT n Achievement group, which was also characteristic of all four goal words but not of the deprivation words.

TABLE III

MEAN RECOGNITION TIMES IN STANDARD SCORES FOR ACHIEVEMENT- AND SECURITY-RELATED WORDS FOR THE UPPER, MIDDLE, AND LOWER THIRDS OF THE n ACHIEVEMENT DISTRIBUTION AS OBTAINED BY COMBINING RANKS ON THE TAT AND ANAGRAMS MEASURES OF n ACHIEVEMENT

n Achievement Groups	N	Achievement Words		Security Words	
		3 − words	7 + words	3 − words	7 + words
High third	12	−.31*	+.29	+.14	+.14
σ_m	..	.16	.13	.15	.10
Middle third	13	−.60	−.10	−.02	+.06
σ_m	..	.12	.08	.14	.11
Low third	11	−.17*	−.08	+.01	−.17
σ_m		.18	.12	.21	.11
t values					
High: Mid-Low	..	.52	2.94**	.75	1.49
Mid: High-Low	..	2.01**	1.58	.49	.52
Low: Mid-High	..	1.55	1.13	.28	2.11**

*One subject omitted whose recognition time for one or more of the − words exceeded six standard score units.

**Near or beyond the 5 per cent level of significance.

combined to attempt to discover what is the most probable shape of the relationship. In this case it seems clear that it is the high group which is different from the other two. In the case of the achievement − words it appears that it is the middle group which is significantly different from the high and low group combined. The t-value is somewhat less than the .05 P level of significance, but a four-way breakdown shows that the V-shaped curve gives way to a U-shaped one which yields a significant amount of nonlinear variance (F = 4.11, P <.05).

The interesting fact is that the security + words also show an increase in sensitivity, particularly from low to medium amounts of n Achievement. There are many possible interpretations of these results, but the one that has recommended itself to us for its simplicity runs somewhat as follows: The group of subjects with moderate n Achievement are primarily concerned to avoid failure. Consequently they are slow to see achievement − words connoting failure and relatively quick to see security + words such as *friend*. The group of subjects with high n Achievement, on the other hand, seem relatively more concerned with actual success itself. Conse-

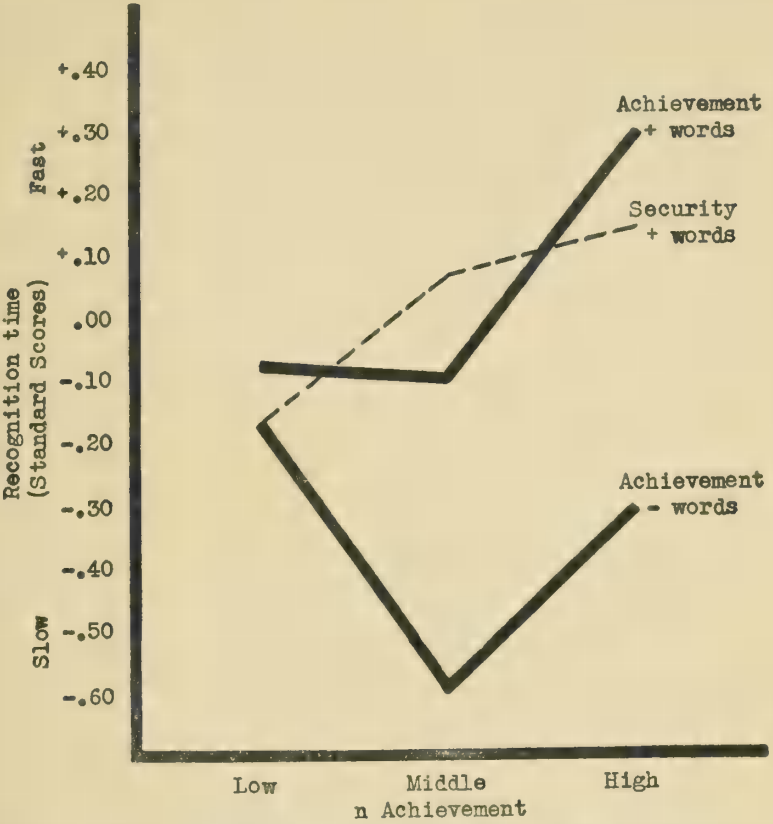

FIG. 1. Mean recognition times in standard scores for achievement- and security-related words plotted for the upper, middle, and lower thirds of the n Achievement distribution as obtained by combining ranks on the TAT and anagrams measures of n Achievement.

quently they are more sensitive to achievement words with a positive connotation, and they do not show to the same extent the middle group's concern to avoid failure, although they are still somewhat less sensitive than the low n Achievement group to the achievement − words and more sensitive to the security + words.

Prerecognition hypotheses. Following the procedure of Postman, Bruner, and McGinnies (5), we analyzed the guesses which the subjects made aloud before correctly recognizing a word. We classified all these presolution hypotheses into the same categories as they did (namely, covaluant, contravaluant, structural, nonsense, and unrelated) for + and − words separately and for the two sets of need-related words. In general our results showed tendencies to support some of their findings (more covaluant hypotheses for the high-need group) and to contradict others (more rather than less contravaluant hypotheses for the high-need group), but our subjects showed such a high degree of variability in the use of such hypotheses that none of these trends proved significant. In fact the only significant trends showed up for the structural hypothesis category for both the achievement- and security-related words. The relevant data are summarized in Table IV. They show first of all a tendency for non-structural hypotheses to increase in frequency from the middle to the high n Achievement groups, but the tendency approaches significance only for the achievement-related words, largely because of the extreme variability in number of such hypotheses used by different subjects.

TABLE IV

Mean Number of Presolution Hypotheses per Subject for Neutral, Achievement, and Security Words for the Upper, Middle, and Lower Thirds of the n Achievement Distribution as Obtained by Combining Ranks on the TAT and Anagrams Measures of n Achievement

n Achievement Groups	N	All Hypotheses	Neutral Word Hypotheses		Achievement Word Hypotheses		Security Word Hypotheses	
			Structural	Other	Structural	Other	Structural	Other
High Third....	12	78.83	4.33	23.92	7.08	16.75	5.08	21.50
σ_m........	..	15.65	.70	6.01	1.06	3.87	.85	5.42
Middle Third..	13	58.46	4.38	14.77	10.54	8.62	3.54	16.62
σ_m........	..	7.86	.90	2.23	1.91	1.22	.43	3.62
Low Third.....	11	61.91	3.91	16.09	9.00	9.44	6.18	16.55
σ_m........	..	9.48	.25	3.38	1.47	2.99	.83	3.11
t values								
High:Middle.	..	1.16	.04	1.43	1.58	2.01*	1.62	.75
Middle: Low.	..	.28	.50	.33	.64	.26	2.83*	.01

*Near or beyond the 5 per cent level of significance.

As far as the structural hypotheses are concerned, there are no shifts among the three groups for the neutral words, as indeed there is no reason to think that there should be. On the other hand there is a significant decrease in the number of security-related structural hypotheses from the low to the middle n Achievement group, which is the opposite of the slight increase noted for the achievement-related structural hypotheses (see Figure 2). Similarly, the trends are in the opposite direction from the middle to the high n Achievement group, the security words showing an increase in structural hypotheses and the achievement words a decrease. In this case it is improbable that the reverse trends for these two types of words would ever go in the same direction ($t = 2.11$, $P < .05$).[3]

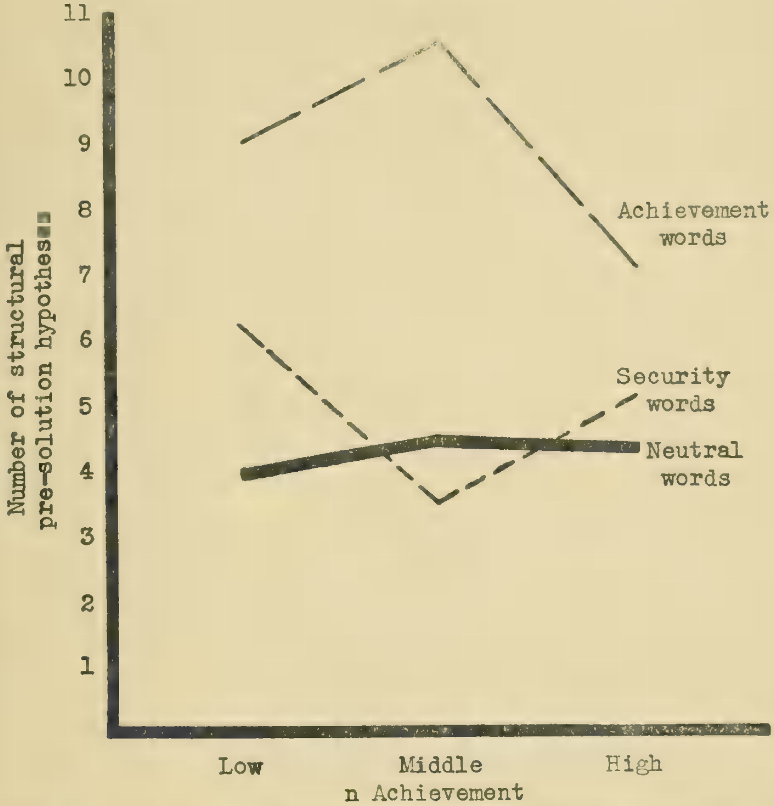

FIG. 2. Mean number of structural presolution hypotheses per subject for neutral, achievement, and security words plotted for the upper, middle, and lower thirds of the n Achievement distribution as obtained by combining ranks on the TAT and anagrams measures of n Achievement.

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[3] The fact that there are many more structural hypotheses for the achievement words than for the security or the neutral words probably reflects the stimulus characteristics of the words rather than their need-related properties. At any rate we attach significance only to the *shifts* among the various groups, although it would be consistent with our hypothesis to expect more such stimulus-

For an explanation of these results we must consider more carefully the nature of a structural hypothesis. Under this heading we classified all responses which were complete words and which were obviously based on the "structural characteristics of the stimulus word" (5, p. 149). In our experiment a frequent stimulus-bound, structural hypothesis was the response *believe* for *achieve* or *master* for *mastery*. Such a response is one that the subject often apparently hopes or believes is correct. It is a "good try" as compared with a more accurate but partial response like *ieve* or a nonsense or unrelated response. Often we found that our subjects who adopted such hypotheses could not understand why they were incorrect. Therefore we would suggest that structural hypotheses indicate a *tolerance for error*. Subjects who make them are satisfied with good approximations and apparently are not motivated to make finer judgments or discriminations.

If this is true, all we need assume further is that good approximations first increase with an increase in motivation and then decrease as motivation becomes very high. Then our results fit with our earlier interpretation of the recognition-time data. The middle n Achievement group is characterized chiefly by a desire to avoid failure and therefore show fewer good approximations in connection with security-related words. They are security-minded, as it were, and show it both in their lessened sensitivity to achievement failure words, in their greater sensitivity to security words, and in their greater vigilance against inaccurate security-related perceptions. On the other hand the high n Achievement group shows some reverse trends, namely, a higher sensitivity to achievement-related stimuli and a greater vigilance in perceiving those stimuli. In both cases faster recognition is associated with fewer approximations which are good but not quite good enough and which, when they occur, delay correct perception. Consequently these data on presolution hypotheses would appear to fit in with our general interpretation that

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bound guesses for achievement stimuli. A possible explanation of some of the shifts might be in the *opportunity* the subjects had to make guesses. For instance, the drop in achievement-related structural hypotheses from the middle to the high group might be a function of the fact that the high group saw the words faster and had less chance to make such guesses. This possibility was ruled out when a check showed that the mean number of exposures for the low, middle, and high groups did not differ materially (although, of course, when the differences were expressed as standard scores they were significant, as shown in Table III).

moderate n Achievement is characterized by a desire to avoid failure and high n Achievement by a desire to achieve success.

Discussion

The primary purpose of this experiment has been fulfilled in showing that our two measures of n Achievement are significantly related to other responses of the organism. At the most general level we have shown that an imaginative and a performance measure of n Achievement are significantly related to the speed with which subjects will recognize need-related words some three or four months after the initial measures were taken. This confirms the hypothesis that our measures of n Achievement are not reflecting simply a temporary motivational state but do in fact represent, approximately at least, the level of achievement motivation a subject maintains over a period of months.

Our data go beyond this elementary but important conclusion to suggest that the central motive state measured previously in imagination and performance also influences the way in which a subject perceives his world. It influences both the speed with which he recognizes certain need-related stimuli and also the kind of perceptual hypotheses which he adopts. More specifically, our data support the hypothesis that an increase in achievement motivation leads first to a concern to avoid failure and then, as motivation gets more intense, to a greater concentration on success. There are a number of facts which support this hypothesis. In the first place with respect to the subjects with moderate n Achievement, they take longer to see such achievement − words as *failure*; they see security + words like *friend* faster, and they adopt fewer security-related structural hypotheses like *fiend* for *friend*. All of this seems to add up to the fact that this group of subjects is security-minded. They appear primarily concerned to avoid doing too badly. They set for themselves, as it were, a *minimum* level of aspiration below which they do not want to fall.

On the other hand the subjects with high n Achievement are success-minded. They see achievement + words like *success* or *famous* faster; they are less likely to use achievement-related structural hypotheses like *believe* for *achieve*, and they show less defensiveness against failure words.[4] It is as if their attention has shifted from a minimum level of aspiration to a maximum. In scholastic terms, these subjects are no longer concerned just with avoiding failure and getting a passing mark; they want to get a *good* mark. In short, there seem to be at least two stages in the way n Achievement influences perception as it develops in intensity.

Is any confirmation of this hypothesis to be found elsewhere? Fortunately, the TAT stories themselves provide a kind of direct check. One of the most significant scoring categories for diagnosing the presence of n Achievement is the one labeled Ga + or Ga −, meaning anticipations of success or failure (4). If our hypothesis is correct, we might expect that the subjects who make up our group with moderate n Achievement would show a Ga − response (anticipation of failure) most often while the group of subjects with high n Achievement would show a Ga + response most often. Consequently, the original scoring of each individual's TAT record was checked for the presence of the two categories and the results tabulated in Table 5. As this table shows our expectation is confirmed.

TABLE V

NUMBER AND PROPORTION OF SUBJECTS SHOWING ANTICIPATIONS OF SUCCESS (GA +) OR FAILURE (GA −) IN THEIR TAT STORIES FOR THE UPPER, MIDDLE, AND LOWER THIRDS OF THE N ACHIEVEMENT DISTRIBUTION AS OBTAINED BY COMBINING RANKS ON THE TAT AND ANAGRAMS MEASURES OF N ACHIEVEMENT

N Achievement Groups	N	Number of Subjects Showing Ga + N	Ga + %	Ga − N	Ga − %
High third	12	10	83%	2	17%
Middle third	13	8	62%	6	46%
Low third	11	2	18%	3	27%

The middle n Achievement group contains a larger percentage of individuals who gave Ga − responses than any other group, whereas the high n Achievement group contains a larger percentage of individuals with Ga + responses. Unfortunately the frequencies are not sufficiently great to yield significant differences, but they are in the direction expected from our hypothesis. It is worth particular note that the same subjects who were slow to recognize failure words more often showed a preoccupation in their stories with the possibility of failure. This suggests that the production of a need-related response and the response to a need-related stimulus are not always perfectly correlated, a fact which confirms an earlier incidental finding with hunger (3) and which suggests one of the reasons why multiple-choice projective tests (involving recognition) are seldom satisfactory substitutes for the regular form of the test (requiring production).

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[4] Actually their continued sensitivity to security + words would suggest that they continue to be interested in security although success had become *relatively* more important.

Further confirmation of the hypothesis that n Achievement may influence behavior in two different ways is to be found in a study of aircraft pilot performance in an experimental cockpit at Cambridge University during World War II. In a task which required the pilot to maintain a pointer on a target for long periods of time, two groups of men could be distinguished, one of which was characterized by a relatively high tolerance for error and by an ability to maintain the performance for a long period of time without disorganization and the other of which was characterized by a much smaller tolerance for error followed often by a rather sudden breakdown in performance.[5] In terms of our present hypothesis it would seem logical to suppose that the men who showed a relatively high error tolerance were operating under a moderate n Achievement which had as its level or aspiration the desire "not to be too far off." The other group of men appear to have been influenced by a more intense n Achievement which has as its level of aspiration the maintenance of the pointer directly on the target. In this particular case the higher n Achievement proved less adaptive in the long run.

Finally we may look to the studies reported by Bruner and associates (1, 5) for further confirmation or contradiction of our hypothesis. It is difficult to make a direct comparison with the results of their studies since they were dealing with variables different from the motivational one studied here. However, they too have found evidence for both a defense and sensitization effect and suggest in at least one study that defense may occur before sensitization. "So long as the situation is not *too* threatening or *too* exacting, avoidance of meaning may be the most economical emotional response. But in situations which are highly threatening and highly exacting, the most adaptive perceptual response is frequently the one which takes most vigilant account of 'reality' " (1, p. 76). However, their experiments have not led to any definite conclusion as to when one process rather than the other occurs. In fact, in one experiment they conclude "we now find sensitization in the presence of 'dangerous' stimulus objects" (1, p. 75), and in another experiment "value orientation . . . leads to *perceptual defense* against inimical stimuli" (5, p. 154). The value of the present study is that it helps define more precisely what the conditions are which lead to defense or sensitization in the recognition process. It shows, in fact, how both of the conclusions about dangerous stimuli quoted above could be correct, depending on what level of motive intensity was involved.

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[5] Cf. D. F. Davis, Pilot error. Air Ministry 3139 A. London: His Majesty's Stationery Office, 1948.

It therefore seems reasonable to conclude on the basis of the internal evidence from this experiment and a comparison with the results of other experiments that increasing n Achievement operates first to motivate the subjects to avoid failure (to orient them around a minimum level of aspiration) and then to concentrate on success (to fix their attention on a maximum level of aspiration).

SUMMARY

Thirty-six male undergraduates, on whom an apperceptive and a performance measure of n Achievement were available, were asked to try to recognize 30 tachistoscopically presented words. They were given successive .01 second exposures of the words at increasing illuminations until they were able to identify them correctly. Ten of the words were neutral, 10 security-related, and 10 achievement-related. The number of exposures taken to see the need-related words was converted into a standard score in terms of the distribution of recognition times for the neutral words for each subject. The subjects' incorrect guesses or presolution hypotheses were also recorded and analyzed. The major findings were:

(1) The subjects with high n Achievement, as determined either by the apperceptive or the performance measure, recognized the achievement-related words with positive (success) connotations (+ words) more quickly than did the subjects with low n Achievement. There were no differences for the achievement-related words connotating failure or difficulty (− words).

(2) When the imaginative and performance measures of achievement were combined to give a more reliable estimate of each subject's

n Achievement, it was found that a breakdown of the resulting combined rank distribution into thirds yielded slower recognition times to the achievement − words for the middle third of the distribution, and faster recognition times to the achievement + words for the higher third of the combined distribution. There was also a reliable tendency for the middle and high thirds to see the security-related + words faster.

(3) An analysis of the presolution hypotheses showed that the middle third of the combined rank n Achievement distribution showed fewer structural hypotheses for the security-related words, and the high third relatively fewer structural hypotheses for the achievement-related words.

These results are interpreted as meaning that the group of subjects with moderate n Achievement are security-minded and chiefly concerned with avoiding failure, or with achieving a minimal level of aspiration, whereas the group of subjects with high n Achievement are concerned more directly with achieving success or attaining a maximum level of aspiration.

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SELECTIVE SENSITIZATION IN AUDITORY PERCEPTION[1]

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The University of Texas

INTRODUCTION

Several studies showing the importance of dispositional "set" in guiding perceptual behavior have appeared in the recent literature. Among the more significant are the studies (2, 3, 4, 5, 6, 7) designed to test the validity of the general proposition that perceptual activity may, under certain conditions, be functionally guided by need, value, stress, tension, and other variables. Several of them have yielded striking evidence of the directive importance in visual perception of these kinds of experience-based determinants.

One of the more important studies is that concerned with personal values as selective factors in perception (7). Using a tachistoscope, Postman, Bruner, and McGinnies (7) presented stimulus material consisting of a list of words which were judged by psychologists to be associated with the value areas measured by the Allport-Vernon (A-V) Study of Values (1). For the sample tested they found that the length of exposure time required to recognize correctly the words associated with the A-V value areas tended to vary inversely with the preference scores on the A-V scale. Although they did not find a direct one-to-one relationship between value score and the "threshold of recognition," the evidence seemed ample to permit the conclusion that there was a corresponding decrease in threshold for words associated with areas in which value was high and a generalized increase in recognition threshold for words associated with areas in which value was low. Their analysis of the quantitative data and of the content of the pre-recognition responses (incorrect responses given to the stimulus) led them to postulate the operation, in perceptual behavior, of three mechanisms: *perceptual sensitization,* in which a lowering of threshold of recognition occurs as a function of high value for the area with which the stimulus is associated; *perceptual defense,* or a heightening of threshold of recognition against inimical stimuli; and *value resonance,* which makes for a persistence of responses which are value oriented, even when the associated stimulus is absent from the immediate environment. In the present study we desire to test the general validity in auditory perception of the first of the three propositions.

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[1] This investigation was made possible in part by the use of the facilities of Radio House at The University of Texas and through the co-operation of the director, Thomas D. Rishworth, and the staff of that installation, to whom we extend grateful thanks.

THE EXPERIMENT

This study is designed to test the hypothesis that experientially derived determinants such as values serve a centrally located integrative system and that this kind of system directively and differentially guides the correct identification of stimuli furnished aurally in the same way that it appears to guide differential identification of such stimuli when presented visually. The procedures followed in testing this hypothesis are described below.

General. The stimulus list used in the present study is the same as the one used by Postman, *et al.* (7). Since their results seemed to show that the visual recognition thresholds for these words were inversely correlated with values, it seemed that the evaluation of the applicability of their hypothesis for auditory recognition could best be made by using these same words. This list of words, which is repeated here for convenience, is shown in Table I.

TABLE I
STIMULUS WORDS REPRESENTING THE SIX VALUE AREAS OF THE ALLPORT-VERNON STUDY OF VALUES[a]

Theoretical	Economic	Aesthetic	Social	Political	Religious
1. theory	2. income	3. beauty	4. loving	5. govern	6. prayer
7. verify	8. useful	9. artist	10. kindly	11. famous	12. sacred
13. science	14. wealthy	15. poetry	16. devoted	17. compete	18. worship
19. logical	20. finance	21. elegant	22. helpful	23. citizen	24. blessed
25. research	26. economic	27. literary	28. friendly	29. politics	30. religion
31. analysis	32. commerce	33. graceful	34. sociable	35. dominate	36. reverent

[a] Reproduced from Postman, *et al.*

In selecting these words, Postman made an effort to standardize their list for both familiarity and for comparability of structure under conditions of visual presentation. This was done in order to minimize the variability in perception that might arise from differences of these kinds. It is probable that the structural characteristics of the word list will be different under aural presentation of the material and that some aspect such as frequency spectrum which varies from word to word should have a differential effect on the ease with which the correct identification of different words is made. An inspection of the word list will show that there is no apparent reason to assume, however, that words within one value area should be easier to recognize in terms of their structural characteristics than the words that represent another value area. Consequently, while there appears to be no efficient way of insuring the desired comparability of stimuli, it seems reasonable to assume that the difficulty of individual words will be equally distributed within the several value categories and that no important systematic error will arise from this source.

As in the Postman investigation, the Allport-Vernon Study of Values was used to measure the values for each of the subjects.

Subjects. The subjects were 22 male senior and graduate students at The University of Texas. Each subject was given the A-V scale as part of an advanced course approximately six weeks prior to the termination of the experiment proper.

Apparatus. The technical facilities of Radio House on the campus of The University of Texas were used in preparing the stimulus material.[2] The word list was recorded in a soundproof studio on two 16-inch Red Label Audiodisc transcription blanks. The recorded voice was that of a senior radio major whose training in speech provided standardization of voice characteristics. Each word in the list was repeated by the announcer ten times at intervals of three seconds. The sequence of the words in the list as they were recorded is that shown in Table I. Although slight variations in the several repetitions of each word probably occurred, it is likely that the utilization of the trained voice minimized this variability. Nevertheless, in order that intensity changes from repetition to repetition and from word to word could be minimized, an RCA Type 86 A-1 limiting amplifier was used in the recording circuit along with an RCA MI 4916 recording filter. The complete recording apparatus is schematically represented in Figure 1a.

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[2] The authors wish to express their appreciation to Gale R. Adkins, Noyes W. Willett, and Julius Donnell for their help in technical details.

Since the method of recording permitted the presentation of the stimulus words to all subjects in the same way, it served to create a constancy of stimulus structure for all subjects, so that differences among the group in their responses to the same word must be attributed to differences in subjects rather than to differences in the stimulus conditions themselves.

For the playback of the records the same facilities were used, and the listening sessions were conducted in the soundproof studio at the station. The records were played through a pair of Brush Hi-fidelity Crystal earphones, and the intensity of the stimulus was controlled with a General Radio Company model 546 Microvolter. A schematic diagram of the playback apparatus is shown in Figure 1b.

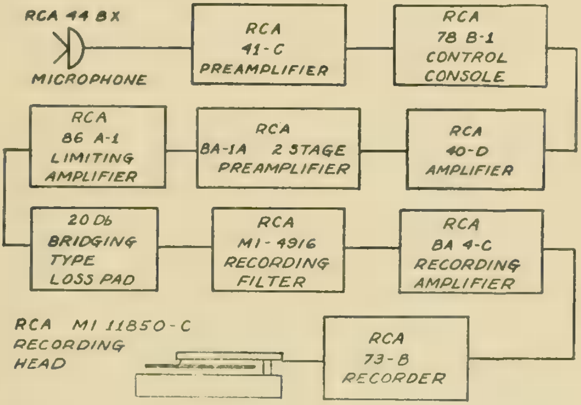
RCA 44 BX
MICROPHONE
RCA 41-C PREAMPLIFIER
RCA 78 B-1 CONTROL CONSOLE
RCA 86 A-1 LIMITING AMPLIFIER
RCA BA-1A 2 STAGE PREAMPLIFIER
RCA 40-D AMPLIFIER
20 Db BRIDGING TYPE LOSS PAD
RCA MI-4916 RECORDING FILTER
RCA BA 4-C RECORDING AMPLIFIER
RCA MI 11850-C RECORDING HEAD
RCA 73-B RECORDER

FIG. 1A. SCHEMATIC DIAGRAM OF THE RECORDING APPARATUS.

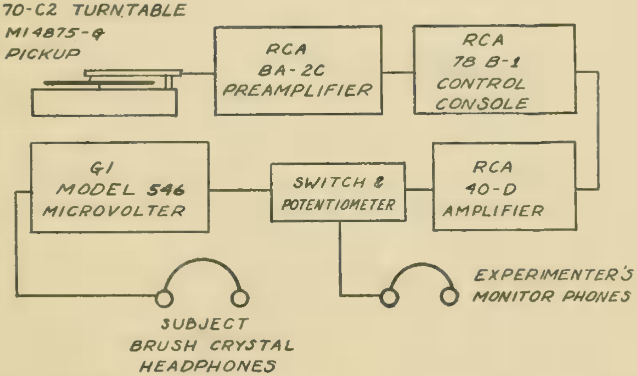

FIG. 1B. SCHEMATIC DIAGRAM OF THE PLAYBACK APPARATUS.

Testing procedure. Each subject was tested for pure tone hearing loss on a standard Western Electric type 6 BP audiometer. All subjects were found to have less than 30 per cent hearing loss over a frequency range from 128 to 8192 cps, with relatively flat audiometric curves being characteristic for the group. Two subjects, NDB and GAF, had tonal lacunae of 32 db and 52 db respectively at 4096 cps, but this loss did not seem to affect adversely their results for recognition of the words.

The subjects were instructed to tell the experimenter what they heard. They were not told the nature of the sounds that they would hear, but they were assured that the sounds could be repeated easily. These instructions served to make the test situation somewhat of a projective one in the sense that they were, as a rule, neither vigilant nor defensive; furthermore, the results of a pilot study indicated that this technique of instruction seemed the most appropriate to elicit the operation of perceptual mechanisms such as those postulated by Postman. We feel relatively certain, for example, that instructions such as "If you mis-recognize the stimulus, you will receive a shock . . . ," would have elicited quite different behavior within the group, and consequently the results must be interpreted with respect to the particular *Einstellung* that was set up by the instructions.

The experimenter, who monitored the records at a constant loudness over separate earphones, had the records played by the studio engineer and, using one ascending series of small increments for each word (since each word had been recorded ten times), varied the voltage across the subject's earphones from a very low voltage until the subject repeated the word correctly. The voltage at which the subject correctly repeated the word was recorded as the threshold score for that word. The voltage was cut back immediately after the subject had made the correct identification so that he never received direct auditory reinforcement by hearing the words at some level well above his threshold of recognition. This procedure was followed for each of the 36 words in the list.

In order that the subjects' responses would not be influenced by the actions of the experimenter (such as his writing verbatim the responses or his turning of the potentiometer dial), a small screen was placed between the experimenter and the subject. The experimenter could observe the subjects, but they in turn could not see what the experimenter was doing.

Results

The data were studied in several ways. The first kind of analysis was introduced in order to study the importance of relevant factors such as hearing deficiency. The second analysis was made to determine empirically whether the method of presentation of the stimuli and collection of the data yielded results that are internally consistent. The third study was done to examine the postulated effect of values upon the thresholds of recognition for the individual subjects. The fourth analysis was designed to determine the general validity of the mechanism of *perceptual sensitization* when applied to these kinds of data. The following paragraphs give descriptions of the analyses just mentioned.

Effect of hearing loss on threshold of recognition. With regard to the effect of hearing efficiency on recognition threshold it is clear that deficiency in the structural determinants of hearing, such as injury to the ossicles, auditory nerve, or other structural mechanisms, will serve to impair the general efficiency of the organism in recognizing auditory stimuli. In addition, it is probable that even the slight differences in hearing acuity which are found in normal subjects will serve to raise or lower the recognition threshold in some proportion to the amount of impairment in pure tone hearing. In examining the audiometric curves for this sample it was found that differences in pure tone hearing acuity did exist among them, though our sample can be considered to have normal, or at least nonclinically impaired, hearing. It should be pointed out, however, that pure tone audiograms seem in no way adequate to account for the large differences in recognition efficiency that exist among individuals. For example, one of the subjects who had pure tone thresholds which showed practically no loss was among the most inefficient in recognizing the words, and conversely, another subject whose audiometric thresholds were among the poorest was among the best in recognizing the words. Of course, in other cases pure tone thresholds corresponded fairly well with recognition thresholds. A number of problems, as yet completely unanswerable, exist with respect to accounting for the determinants of these large inter-individual differences in recognition thresholds. Attention will be given to these problems in a more extensive series of studies now getting under way. Suffice it to say that, as a group, the hearing of this sample was essentially normal.

At the moment, however, we are not concerned with absolute differences considered on an inter-individual basis since they are within the "normal" range; rather, we are concerned with differences in recognition efficiency on an intra-individual basis as they relate to values. We want to know whether or not an individual A, who has a high theoretical value and a low religious one, for example, recognizes theoretical words at a low intensity level and religious ones at a higher intensity level than that at which he correctly perceives the theoretical words. There is no reason to assume that structural anomalies should make for inefficiency in hearing words in one value area and for efficiency in hearing words in another value area; the effect should be "constant" across the several families of words, and, if so, it could not serve to account for the intra-individual differences that exist.

It may be said, in view of these considerations, that, although hearing acuity undoubtedly has some effect on the threshold of recognition for the stimulus material presented, the kinds of inter-individual differences that we have found cannot be invoked to account for the great area-by-area intra-individual differences in recognition thresholds that were observed. We now turn to analysis of the internal character of the recognition test itself.

The reliability of the recognition test and of the A-V Scale. To determine the reliability of the recognition data the correlation between odd versus even items was computed by pairing the odd and even items within each value category and then summing through the six value categories for each subject and computing the coefficient across the sample. This coefficient is +.92 ± .04 (corrected to +.96 ± .02), a value which indicates a satisfactorily high degree of internal consistency in the performances of this group. In view of the magnitude of this correlation it may be concluded that the test for recognition thresholds gives adequately consistent results for our purposes.

The A-V Study of Values may also be considered from the standpoint of this kind of internal consistency. For such an analysis the reader is referred to the test manual (1). It is sufficient to mention here that the reliability coefficients that have been reported range from +.60 to +.87.

Having thus considered the effect of hearing acuity on the recognition thresholds and having dealt with the problem of the internal consistency of the recognition thresholds and values data, we turn to the central problem: Does the postulated mechanism of *perceptual sensitization,* operating through the individual's value system, serve to guide selectively the perception of stimuli aurally presented in the same way that it appears to guide the perception of stimuli presented visually?

Analysis of recognition thresholds for individuals. In order to determine the relationship between values and recognition thresholds, individual-by-individual, two kinds of analyses were undertaken: the first was designed to describe, in profiles similar to those furnished by Postman, Bruner, and McGinnies, the individual performances, while the second was introduced in order to present some numerical index of the strength of the relationship between values and recognition thresholds on an individual basis.

The first analysis, presented in Figure 2, consisted of graphing the profiles for recognition thresholds by value areas in comparison with the scores on the A-V scale. The word list was divided into the six groups of words that are shown in Table I. Mean recognition threshold scores were computed for each subject for each group of six words, representing the A-V areas, and these values were plotted on the same graph with the A-V scores. In order to make recognition threshold scores directly comparable with the scores on the A-V scale, the recognition threshold scores were plotted inversely to the A-V scores. Thus if the profiles within a graph show great similarity, it indicates that the threshold values are low for areas in which value is high, and vice versa. An inspection of these graphs reveals a considerable amount of variability, but it also shows that a marked tendency exists for high value words to be recognized at lower intensities than the low value words are recognized, and that the converse is true for low value words. In a few cases, such as the graphs of CHB, LLB, and BFJ, there is a remarkable correspondence between the two profile plots, while in

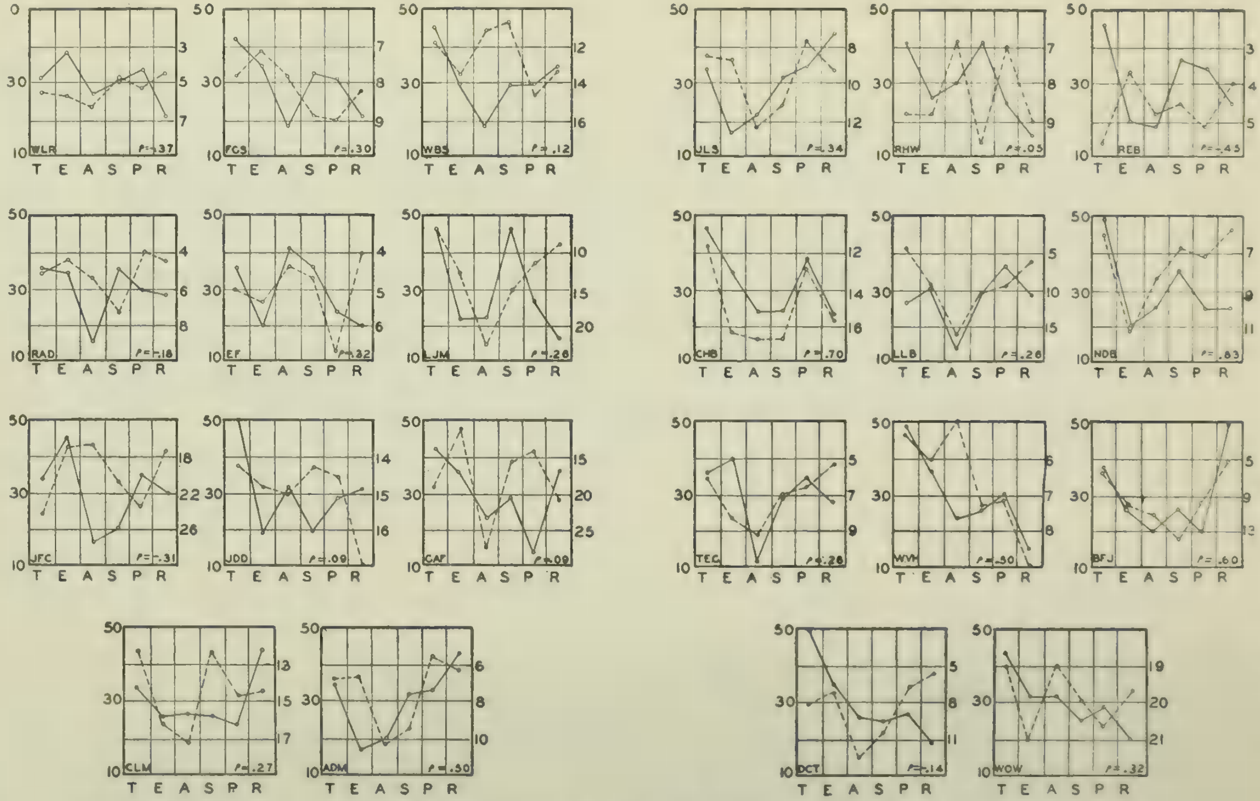

FIG. 2. PROFILES OF VALUE SCORES AND OF RECOGNITION INTENSITIES OF THE INDIVIDUAL SUBJECTS.

The A-V value categories tested are shown on the abscissa. Value scores are plotted on the left hand ordinate. Average recognition intensities for the words representing these values are plotted on the right hand ordinate. The solid lines represent the A-V value scores, while the dotted lines represent recognition intensities. The rank-order correlations between "amount of value" and recognition threshold are also shown for each individual.

others, such as the graphs of JFC, REB, and RAD, there seems to be a reversal of this trend.

The second analysis consisted of computing rank order correlations between the average threshold for each value area and its corresponding A-V score, individual by individual. These give an index of the strength of the relationship between value levels and recognition thresholds for each subject. The rank order coefficients, which are shown in the lower right hand corner of each plot, range from $-.45$ to $+.83$, with an average (computed by converting each to the z function and reconverting to ρ) of $+.26$.

In order to determine whether any positive relationship exists through the group as reflected in these rank order coefficients, the χ^2 test for the null hypothesis was performed on the distribution of ρ's. In these data 5 coefficients are negative, but 17 of them carry positive signs. If the association between recognition thresholds and values were not different from chance, one would expect 11 of the 22 coefficients to carry positive signs and 11 to carry negative signs. The χ^2 test of this hypothesis yields a value of 6.54 which is significant at the 2 per cent level. We may conclude, therefore, that even though considerable variability in the relationship between recognition threshold and value, as seen in the plots, exists among our subjects, the relationship between these two variables is in general a positive one. In order to obtain information that better describes these relationships for the sample as a whole, we now present the results of the group analysis.

Group analysis. In this analysis, we first computed the average threshold of recognition in each of the six A-V areas for the total sample tested, and plotted these average values on a single graph similar to the ones presented for the individuals. Along with the recognition thresholds we plotted the average A-V scores for the group in each of the A-V areas on the same graph. This profile, which is presented in Figure 3, shows the generality of the trends presented for individuals in Figure 2. The values of the left-hand ordinate are standard scores (z) derived from the distribution of group average recognition threshold scores, and the values on the right-hand ordinate are standard scores (z) derived from the distribution of group average A-V scores across the six value areas. The rank order correlation between the corresponding points from these two sets of data is +.80. These data seem entirely consistent with the hypothesis of *perceptual sensitization;* however, a more detailed analysis is presented in the following paragraphs.

The second analysis of the group data consisted of graphing the average recognition threshold for the group in each of the value areas as a function of value rank rather than as a function of specific value area. That is, we wished to determine whether or not a significant increase in recognition threshold occurred as a function of decrease in value rank rather than as a function of specific value score. For this analysis each subject's A-V scores were ranked from highest to lowest, and the recognition threshold for each of the corresponding value ranks was recorded. The group average recognition threshold for each of the six value ranks was then computed and plotted. This graph is given in Figure 4, an inspection of which shows that as value intensity decreases (from value rank 1 to value rank 6), there is a corresponding increase in group average recognition thresholds for the words associated with those

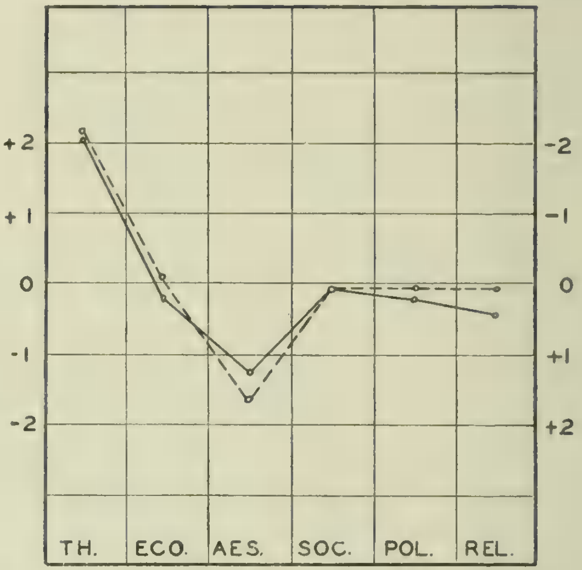

FIG. 3. PROFILE OF AVERAGE A-V SCORES AND RECOGNITION INTENSITIES FOR THE GROUP.

Value scores are expressed as standard scores $(\frac{x}{\sigma})$ and plotted on the left-hand ordinate. Recognition intensities are also expressed as standard scores $(\frac{x}{\sigma})$ and plotted on the right-hand ordinate. The solid line represents the A-V values; the dotted line represents the threshold intensity values.

value ranks. These data conform fairly well to a straight line function. This finding confirms the earlier conclusion that stimuli representing high value areas are perceived at lower recognition thresholds than the stimuli that represent low value areas. Thus, under conditions of aural presentation, the postulated mechanism of perceptual sensitization seems to apply as it did for the visually presented stimuli studied by Postman.

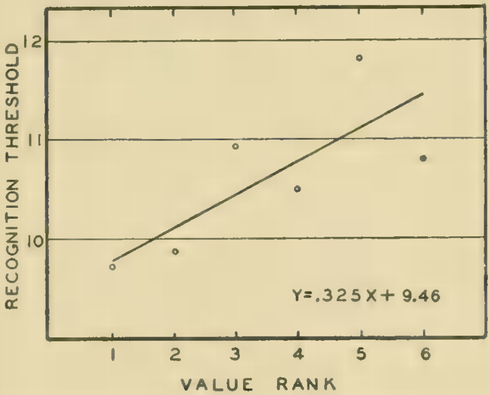

FIG. 4. AVERAGE RECOGNITION THRESHOLDS FOR THE WORDS REPRESENTING THE SIX VALUE AREAS ARRANGED IN RANK ORDER.

In order to test the possibility that these relationships might have occurred as a product of chance, tests for the significance of differences between means were computed between the recognition thresholds for each value rank and each other value rank. These results are summarized in Table II, which includes the value of *t* for differences between the observed means for each value rank as well as for the hypothetical means as defined by the straight line which has been fitted to the data. Table II shows that as value rank proceeds from high to low (1 to 6), recognition thresholds become significantly different and progressively more so, the greater the differences in value. Again, these findings agree very closely with those of Postman, and since the differences in recognition threshold are so closely and significantly related to differences in value rank, it may be concluded that *perceptual sensitization* is confirmed as valid for aurally presented stimuli.

DISCUSSION

The evidence from this study and from the one conducted by Postman seems to show that value orientation is associated with recognition efficiency in two of the sense modalities. Can these results be interpreted within a psychologically meaningful context? Two explanations which appear logical are discussed below.

TABLE II

VALUES OF t (CORRECTED FOR CORRELATION) FOR DIFFERENCES BETWEEN THRESHOLDS AS A FUNCTION OF VALUE

		1	2	3	4	5	6	
	1		.67 *.30*	1.51 *.10*	2.45 *.025*	1.78 *.05*	3.20 *.01*	
	2	.03 *.50*		.30 *.40*	1.51 *.10*	1.59 *.10*	1.78 *.05*	
Observed Threshold Values	3	.28 *.40*	.97 *.20*		.70 *.30*	1.10 *.15*	1.40 *.10*	Theoretical Threshold Values
	4	.19 *.45*	1.42 *.10*	−.94 *.20*		.56 *.30*	.92 *.20*	
	5	2.88 *.01*	2.36 *.025*	1.51 *.10*	2.33 *.025*		.25 *.40*	
	6	2.11 *.025*	1.25 *.15*	.19 *.45*	.42 *.35*	.77 *.25*		

The values in the left half of the table are those for the observed results in Figure 4; the values in the right half are those for the results as described by the straight line in Figure 4. The figures in roman type are the values of *t*, while the italicized figures are the probabilities of the *t*'s.

First, value orientation creates within the individual a framework of meaning which in turn serves to focus the perceptual mechanisms more sharply on stimuli which are associated with high value areas than on other kinds of stimulus material. This sharpened focus in turn lowers the perceptual threshold for value-associated stimuli. Conversely, in low value areas the uncrystallized focus of meaning appears to raise the perceptual threshold for stimuli associated with such low value areas.

Secondly, value orientation creates within the individual a greater familiarity (associative frequency) with value-associated stimuli than with other kinds of material. This familiarity in its turn serves to lower perceptual threshold for the more familiar stimuli in greater proportion than for non-familiar stimulus material. On the other hand, the perceptual threshold for material which is not value-associated, and therefore unfamiliar, will be raised.

That these two processes are interactive as determinants of perceptual sensitization seems probable. Meaningful stimulus material becomes familiar in the process of assigning meaning to the material, and familiar stimuli take on meaning in the familiarization process. An important aspect of this interaction is the relative importance of meaning in perceptual sensitization when familiarity is held constant as compared with the importance of familiarity when meaning is held constant or minimized. The contributions attributable to these two sets of determining conditions will be analyzed in a series of studies planned for the immediate future.

Summary and Conclusions

The aim of this study was to test the validity of the mechanism of *perceptual sensitization* by studying the effect of value and other variables on the perception of aurally presented stimuli. Thirty-six words, the ones used originally by Postman, Bruner, and McGinnies, selected to represent the six value areas measured by the Allport-Vernon Study of Values, were recorded and presented aurally under controlled conditions to a group of 22 college students whose pure tone hearing losses were all within normal limits. Recognition threshold scores for each group of words in the six value areas were computed and compared with the subject's scores on the A-V Study of Values.

The results show clearly that value plays a selective role as one determinant of recognition efficiency. In general, subjects perceived words representing their high value areas at significantly lower intensity levels than they perceived words which represented their low value areas. This finding for auditory perception, which is consistent with that furnished by Postman, Bruner, and McGinnies for visual perception, supports the general assumption that some mechanism of *perceptual sensitization* operates to differentially raise or lower recognition thresholds.

A discussion of the psychological meaning of these data suggests that decreases in recognition threshold may come about through the operation of factors related to familiarity with valued stimuli, through the effects of assigning more precise and therefore more meaningful definitions to valued stimuli, or through the interaction of these two processes. The contribution attributable to meaning and that attributable to associative frequency cannot of course be determined from these data.

In terms of the original hypothesis it appears that experientially derived determinants such as values serve a centrally located integrative system which directively and differentially guides the recognition of certain kinds of auditory stimuli.

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